When Income Effects are Large: Labor Supply Responses and the Value of Welfare Transfers

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Abstract

The effect of income on labor supply is a parameter of great importance for both theory and policy analysis. In this paper, I provide new estimates of the income effect of welfare transfers on individual labor supply. I leverage novel social security administrative data on the universe of survivor insurance payments in Italy, and useful quasi-experimental variation in the benefits received by surviving spouses on the basis of their spouse's death date. I implement a regression discontinuity design in spousal death date to identify the effect of unearned income on labor supply, earnings and program substitution. Benefit losses trigger tantamount increases in earned income, implying a marginal propensity to earn out of unearned income of approximately -1. Extensive-margin responses – in the form of both increased labor-market entry by younger survivors and delayed retirement by older survivors – emerge as the main driving force behind the income response. Program substitution also appears to be a relevant margin of adjustment. I consider alternative explanations for the large income response. Finally, I discuss the normative implications of my findings. I propose a revealed-preference approach to estimate the value of transfers based on participation responses. I demonstrate that large participation responses to realized benefit drops are revealing of large implicit valuations of welfare transfers in the widowhood state, and of substantial welfare gains from more generous survivor insurance.

JEL codes: H55, I38, J22.

Keywords: Income effect, Labor supply, Evaluation of welfare gains.

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1 Introduction

The effect of income on labor supply is a parameter of great importance for both theory and policy analysis. Theoretically, the income effect provides the link between the uncompensated and the compensated elasticity through the Slutsky equation. Estimates of the income effect, combined with estimates of the uncompensated wage elasticity, are thus useful to back out the compensated elasticity, which is itself a key parameter for optimal tax policy design and for the calibration of business cycle models. From a policy perspective, income effects are central to the evaluation of a broad set of policies involving income transfers, such as social insurance programs, public pension schemes and tax policies. Income effects are also important for welfare analysis, since they are directly related to the marginal utility of consumption (Chetty [2004], Chetty [2008]). Recent work has shown that income effects are also key inputs of standard balanced-growth theory, because they help rationalize the secular decrease in hours worked observed in many developed and developing countries (Boppart and Krusell [2016]).

In spite of their importance for economic analysis, we still know surprisingly little about income effects, especially in the context of welfare transfers. This is mostly due to the fact that the effect of income on labor supply is hard to identify. Identification is often complicated by the fact that social insurance, and tax and benefit programs generally involve simultaneous transfers of income and changes in work incentives, which make it hard to separately identify income and substitution elasticities. The ideal experimental setting for identification would require to randomly allocate substantial lump sums to individuals for a long period of time. In practice, such ideal experiment is hard to come by. For this reason, income effects have been typically either assumed away, or calibrated to recover compensated elasticities. Most quasi-experimental estimates of income effects are based on transfers that are either too modest to trigger a response, or relatively short-lived implying that observed responses may be substantially attenuated by optimization frictions (Pencavel [1986], Blundell and MaCurdy [1999], Kimball and Shapiro [2008], Marinescu [2018]). It is therefore still unresolved whether existing estimates of income effects are indeed capturing the true effects of income on labor supply, especially in relation to welfare transfers.

In this paper, I provide novel estimates of the income effect of welfare transfers on individual labor supply, earnings and total income. To do so, I exploit a unique research setting in the context of the Italian survivor insurance scheme. Survivor insurance is a public scheme providing a pension benefit to surviving spouses of deceased retirees and workers. The benefit is computed as a fraction of the deceased's pension and starts from the beginning of the month following the death.[1] I take advantage of a policy change that introduced an exogenous, large and permanent discontinuity in the fraction of the deceased's pension received by survivors on the basis of their spouse's death date. Specifically, the reform decreased the fraction of the deceased's pension received by survivors

[1] Entitlement to the benefit is lost upon remarriage. It otherwise continues until death.

whose benefit started on or after September 1, 1995, generating a discontinuity in the expected lifetime benefit of €100,000 (or 31 percent of the mean in the pre-reform regime) and *de facto* introducing two parallel benefit regimes of exogenously different generosity that would then coexist for a long time.[2]

Using newly released, rich administrative data on the universe of survivor insurance payments and survivors' contributory histories from the Italian Social Security (INPS), I implement a regression discontinuity design in the spousal death date – which is equivalent to the benefit start date – and compare the long-term outcomes of otherwise identical individuals receiving benefits of different generosity for a long time, in order to identify the income effect of transfers on individual labor supply and other economic behavior. The long-run identifying variation generated by the benefit reform offers a unique window on the long-run behavioral responses to a permanent reduction in benefits. Specifically, the research setting allows to estimate long-run effects that are plausibly not attenuated by short-run optimization frictions. Also, by comparing treated and control individuals similarly affected by the loss of a spouse, the identification strategy implicitly controls for the confounding role of state-dependent preferences. Finally, while most existing estimates of income effects are based on benefit expansions, this setting allows me to explore the effects of a benefit loss.[3] In this respect, it is often implied that the effect of a positive and a negative income shock would be symmetric, but this need not be the case if, for instance, agents are loss averse or have sticky consumption habits.

I find that survivors fully offset the benefit loss by increasing their earnings and, as a result, do not experience any drop in disposable income. Specifically, in the fifteen years after their spouse's death, survivors affected by the reform lose on average €2,000 per year, which is equivalent to a 21 percent drop relative to old-regime surviving spouses. In response, they increase their average annual earned income by a quantity equal to the benefit loss. This translates into an estimated marginal propensity to earn out of unearned income (MPE) of approximately -1.0, indicating that earned income increases one-for-one with decreases in unearned income.[4] I document substantial heterogeneity in the income effect by the relative severity of the benefit loss.

I probe the large income response by examining its underlying mechanisms. Firstly, I decompose the earned income response along three margins: labor force participation, hours worked and the wage rate. Labor force participation is the main driver of the income response: a loss of €1,000 in benefits increases labor supply by 4 percentage points on average in the fifteen years after the spouse's death – an effect equivalent to 7 percent of the mean in the control group. The

[2] In the empirical analysis, I restrict the sample to individuals aged 55 and under at the time of their spouse's death. The expected lifetime benefit drop is computed on this sample.

[3] It is likely that individuals in the new regime expected higher survivor benefits, especially given that the reform was little anticipated.

[4] The income effect – or marginal propensity to earn out of unearned income – is measured as the change in earned income for a unit change in unearned income.

participation response is driven by both increased labor-market entry by younger survivors and delayed retirement by older survivors. Hours worked and the wage rate are found to have a muted response to changes in the benefit. Secondly, I uncover interesting dynamic patterns in the labor supply response: participation responses are silent in the two years after the spouse's death and then grow steadily larger over time, reaching a differential of 18 percent after 15 years. The observed pattern in the years immediately after the death is likely due to grief. The overall dynamic is also consistent with the notion that optimization frictions, such as adjustment costs, attenuate responses in the short run and fade away over time. The dynamics of hours worked and the wage rate are flat throughout the 15 years following the spouse's death, indicating no intensive-margin adjustments and suggesting that work experience, human capital accumulation and effort have limited returns in the context under study.

I investigate program substitution responses as an additional margin of adjustment in response to the income drop.[5] I find that survivor benefit reductions trigger a statistically significant increase in the take-up of paid family leave and unemployment insurance benefits. The magnitude of both effects is large as a percentage of the mean in the control group. I interpret the increase in paid-family-leave take-up as an indication that surviving spouses who increase their labor supply may be doing so under substantial work-time constraints due to care duties. The increase in unemployment insurance take up may instead suggest that individuals are willing to pay the cost of unemployment stigma to increase disposable income.

Finally, I discuss the normative implications of my findings. A central result of this paper is the large labor supply and taxable income response to a negative income shock. Why is the income effect that I estimate so large? On the one hand, if an individual increases labor supply sharply in response to a benefit drop, that lost income must be of high utility value. On the other hand, a substantial labor supply response may arise if the cost of adjusting labor supply is small. Understanding which of these two alternative mechanisms prevails is important for welfare analysis. I first provide evidence of there being adjustment costs associated to the observed labor supply response. I show that the participation response to a given benefit drop is a negative function of the contemporaneous unemployment rate in the region in which the survivor resides. Since the unemployment rate is arguably positively correlated with the utility cost of labor – either because finding a job requires more search effort or because keeping an existing job requires more on-the-job effort –, this evidence is consistent with the notion that adjustment costs are non-zero in the context analyzed.

I then propose a new methodology to estimate the value of transfers, based on participation responses to benefit losses. I demonstrate that survivors' labor supply responses to a realized drop

[5]Program substitution refers to a change in take-up of other social assistance and social insurance programs (conditional on eligibility) in response to a change in a given program's generosity (Inderbitzin, Staubli and Zweimüller [2016]).

in benefits reveal their implicit valuation of the benefit in the widowhood state, as measured by the gap in the marginal utility of consumption between the low-benefit and high-benefit regime. Intuitively, the extent to which individuals increase work effort in response to a drop in unearned income reveals, *ceteris paribus*, the consumption value that such lost income would have provided. Hence, larger responses must mean that the lost income is highly valued and that there are large welfare gains from recouping it. I provide conditions under which the semi-elasticity of labor force participation to an unconditional transfer scaled by the semi-elasticity of labor supply to the wage rate can be used to evaluate the marginal welfare gains from increased survivor insurance generosity. I estimate a marginal welfare gain of 0.5, which implies that the marginal utility of consumption is 50 percent higher among widow(er)s in the low-benefit regime as compared to widow(er)s in the high-benefit regime. This is in the higher end of the range of existing estimates of the value of social insurance, unemployment insurance in particular. It follows that widowhood is a state with a high marginal utility of consumption and that increasing the generosity of survivor insurance would generate substantial welfare gains.

Whilst there is a selection issue of studying survivor insurance benefits, nonetheless I believe there is scope for generalizability of the findings obtained in this context. Individuals in my sample are spouses – prevalently women – who become widow(er)s in their mid forties. Single-parenthood, and single-motherhood in particular, are among the states most at risk of income insecurity and single parents make up a large proportion of welfare recipients of programs such as Earned Income Tax Credit (EITC), Aid to Families with Dependent Children (AFDC) and Temporary Assistance to Needy Families (TANF). Hence, to the extent that widow(er)s in my sample can be representative of single parents in general, my findings can be relevant to a larger set of public policies in the US and in Europe. Losing a spouse at a young age is a low-probability and relatively unpredictable event against which households are likely to be limitedly insured. In this respect, my estimates of the labor supply and income response are likely to provide an upper bound of what would be expected for "shocks" that are easier to anticipate and insure *ex ante*, such as own or spousal job loss and skills obsolescence. Finally, the elasticity of labor supply may differ between marriage and widowhood, due to leisure complementarities between spouses (Goux, Maurin and Petrongolo [2014]), a potentially increased desire to engage in working activities due to loneliness during widowhood, or the (in)ability to share family-related duties with a partner. To the extent that labor supply elasticities are higher (lower) in the widowhood state, the effects estimated in this paper are likely to provide an upper (lower) bound for what would be expected for married individuals.

Related literature and main contribution. The findings in this paper inform a long-standing line of research on the income effect of welfare transfers on labor supply. Early studies of the US negative income tax (NIT) and the Canada Mincome experiments that took place simultaneously in the 1970s tend to find negative, but small and statistically insignificant effects of income guarantees on employment and hours worked (Robins [1985]; Burtless [1986]; Ashenfelter and Plant

[1990]; Hum and Simpson [1993]). Whilst the combination of wealth transfers and marginal tax rate changes that characterizes NIT-like experiments makes it complex to disentangle income and substitution effects, consensus estimates set the income effect between -0.10 and 0.00. The ability to draw conclusions from NIT experiments is however limited by data collection issues such as selective attrition and earnings misreporting, and by the short duration of the programs, which raises concerns that observed responses may be attenuated by frictions.[6]

To compellingly isolate income effects, subsequent studies have examined the few existing examples of unconditional cash transfers and lottery wins, as both settings do not entail alterations of the tax structure faced by individual recipients. Studying the Eastern Band of Cherokee Indians Casino Dividend, Akee et al. [2010] find results consistent with a zero income effect four years after the start of the payments. Similar results are found in the context of the Alaskan Permanent Fund Dividend (Jones and Marinescu [2018]). However, in both studies, dividend payments may be correlated with increased job opportunities, implying that micro-level income effects may be compensated by opposite-signed macro labor demand effects. Studies of lottery winners in Massachusetts (Imbens, Rubin and Sacerdote [2001]) and Sweden (Cesarini et al. [2017]) both estimate marginal propensities to earn out of lottery wins of -0.10. Being closest to the ideal experiment for the identification of a causal effect, lottery studies provide internally valid and credibly identified estimates of wealth effects on labor supply. There are, however, concerns about the generalizability of their findings to other contexts, due to the selected nature of both the population of lottery players and the type of wealth shock that lottery wins constitute.[7,8] I contribute to this literature by providing well-identified estimates of the income effect from a large and permanent drop in unearned income, in the long term and in the context of publicly provided benefits.

This paper is also related more broadly to the literature on the labor supply and program substitution effects of social insurance programs, such as disability insurance (Bound [1989]; French and Song [2014]; Kostol and Mogstad [2014]; Autor et al. [2016]; Deshpande [2016*a*]; Deshpande [2016*b*] Autor et al. [2017]), health insurance (Garthwaite, Gross and Notowidigdo [2014]), earned income tax credits (Eissa and Liebman [1996]; Saez [2010]) and retirement wealth (Krueger and Pischke [1992]; Gelber, Isen and Song [2016]). Two of these studies provide estimates of the income

[6] Price and Song [2018] study the long-term effects of NIT experiments taking place in Denver and Seattle in the 1970s on individual outcomes up to four decades after the programs ended. Treated individuals have lower post-experimental annual earnings and a higher propensity to apply for disability insurance compared to individuals in the control group, suggesting that income support may have important dynamic effects.

[7] The cited studies find that lottery players tend to differ in their observables from the general population: specifically, they are more likely to be male, older, less educated and with lower earnings. Lottery players may as well differ in their unobservable characteristics, such as the degree of risk aversion. Moreover, to the extent that lottery wins and welfare transfers are not fungible (Thaler [1990]), lottery-based estimates need not be representative of responses to welfare transfers or other public policy schemes. Finally, the magnitude of lottery wins is in most cases small.

[8] Unconditional cash transfers have been widely studied in developing countries. Recent surveys of the existing literature are unanimous in concluding that cash transfers had no detrimental effects on employment (Bastagli et al. [2016]; Banerjee et al. [2017]).

effect that are much larger than consensus estimates in the literature and more in line with the findings in this paper. Specifically, Deshpande [2016*b*] estimates a parental earnings response to the loss of Supplemental Security Income payments of approximately -1.4, while Gelber, Isen and Song [2016, 2017] estimate an upper bound of the elderly earnings response to the Social Security "Notch" of -0.6 for men and -0.89 for women.[9]

This paper is also partly related to a large literature on the divergence between steady-state macro and micro elasticities of labor supply.[10] Macroeconomic models of cross-country variations in hours worked imply elasticities that are much larger than those estimated using micro-level sources of identifying variation. Optimization frictions (Chetty [2012]) and the indivisibility of labor (Rogerson [1988], Ljungqvist and Sargent [2007], Rogerson and Wallenius [2009]) have been identified as the two main factors that can reconcile such divergence. As previously argued, the long-run variation offered by the 1995 reform is useful in identifying a parameter estimate likely not attenuated by short-run optimization frictions. I show that the – arguably frictionless – micro elasticity that I estimate is indeed statistically compatible with macro elasticities of labor supply and with the steady-state frictionless hour elasticity identified in Chetty [2012]. I also discuss the relationship between my findings and macroeconomic models of indivisible labor.

Finally, this paper contributes to a growing body of work that attempts to evaluate the welfare gains of social insurance using empirically estimable "sufficient statistics". Applied predominantly in the context of unemployment insurance, consumption-based approaches use consumption responses to unemployment or job loss (combined with measures of the coefficient of relative risk aversion) to infer the value of unemployment insurance (Baily [1978]; Gruber [1997]; Chetty [2006*a*]). The main limitation of consumption-implementation approaches is their reliance on consumption data, for which availability is limited and often partial, and where there are issues of mismeasurement and difficulties with assignment to individuals within a household. From a theoretical standpoint, the approach also relies on the potentially strong assumptions of state-independent preferences and no anticipation effects. Partly in response to these limitations, recent work has developed revealed-preference, optimization-based approaches that exploit behavioral responses to estimate the welfare gains from social insurance. While most work has been done in the context of unemployment insurance (Shimer and Werning [2008]; Chetty [2008]; Landais [2015]; Hendren [2017]), similar approaches have been developed for social insurance against fatal and non-fatal health shocks (Fadlon and Nielsen [2018]; Dobkin et al. [2018]).[11] I contribute to this literature by providing a simple revealed-preference method based on *within*-state participation responses to

[9] Interestingly, both those and this study exploit exogenous *reductions* in benefits relative to the *status quo ante* (contrary to the majority of studies in the literature), hinting at potentially asymmetric responses to benefit gains and losses (Deshpande [2016*b*]).

[10] See Chetty et al. [2011] for a review of this literature.

[11] In recent work, Landais and Spinnewijn [2018] propose a revealed-preference approach based on marginal propensities to consume that allows for state-dependent preferences and accounts for unobserved margins of adjustment.

benefit losses that allows for state dependence and is applicable to a broad class of public policies involving income transfers. Moreover, because it requires labor supply rather than consumption data, the revealed-preference approach that I propose has the advantage of being widely applicable given the increasingly large availability of detailed data on individual labor supply from administrative and other sources.

The paper proceeds as follows. Section 2 outlines the institutional details of the Italian survivor insurance scheme and of its 1995 reform, and discusses the expected effects of the reform on individual labor supply. Section 3 describes the INPS administrative data and illustrates the empirical strategy. Estimates of the effect of survivor insurance benefits on total income, labor supply and program substitution are presented in Section 4. Section 5 examines the external validity of the findings and their relation to theories of labor supply. Section 6 discusses the normative implications of the findings and provides a theoretical framework to evaluate the welfare gains from increased survivor insurance generosity. Section 7 concludes.

2 Institutional Setting and Conceptual Framework

2.1 Background on the Italian Survivor Insurance Scheme and its 1995 Reform

The largest across OECD countries, the Italian survivor insurance scheme amounts to 2.4 percent of GDP and involves up to 4.4 million recipients in 2017 (INPS [2018]). The scheme provides benefits to the relatives of deceased retirees and disability-insurance recipients entitled to a state pension (in which case the survivor pension is called *pensione di reversibilità*), and of deceased workers who have a minimum number of accrued weeks of compulsory contributions towards their state pension (in which case the survivor pension is called *pensione indiretta*).[12]

The benefit is universally provided to the following surviving relatives: the surviving spouse, even if separated or divorced provided that alimony rights have been granted and that the spouse has not remarried; dependent children, who are minors, incapacitated or students (including university students); fully-dependent minor grandchildren; absent the above, dependent parents aged 65 and over, and siblings, who are not simultaneously entitled to other social security benefits. The benefit starts at the beginning of the calendar month following the death date, irrespective of when the application is filed. For surviving spouses, entitlement to the benefit ends once they remarry; for

[12] In order to qualify for survivor insurance (*pensione indiretta*), deceased workers who have not yet retired at the time of their death, must have accrued a minimum of 780 weeks of contributions or a minimum of 260 weeks of contributions, of which 156 in the five years prior to death. In case these requirements are not met, survivors are entitled to a one-off lump-sum payment. Survivors of deceased workers with at least one year of contribution in the five years prior to their death and whose contributory history started on or before December 31, 1995 are entitled to a benefit equal to 45 times the amount of their contributions, up to a cap of €2,979.90. Conditional on having an income below the social assistance threshold, survivors of deceased workers whose contributory history started after December 31, 1995 are entitled to a one-off payment equivalent to the number of years of contributions of the deceased times €448.00.

dependent children and grandchildren, once they turn 18 or lose their incapacitation status; for parents and siblings, once they lose their dependency or incapacitation status, or once they become entitled to other social security benefits.[13] Dependent children and grandchildren aged 18-21 who are high-school students and not working are entitled to the benefit up to age 21. University students up to age 26 are also entitled to the benefit, provided that they are not working.

The amount of the benefit is computed as a percentage of the pension that the deceased was or would have been entitled to at the time of death.[14] Table 1 summarizes the replacement rates – i.e. the percentage of the deceased's pension received by surviving relatives – for different types of survivors. As reported in the first column, a spouse without dependent children or grandchildren receives 60 percent of the pension of the deceased, a spouse with one dependent child 80 percent and a spouse with two or more dependent children 100 percent. Absent the spouse, the replacement rate for a sole dependent child is 60 percent, for two dependent children 80 percent and for three or more dependent children 100 percent. Absent the spouse, children or grandchildren, dependent parents and siblings are entitled to 15 percent of the deceased's pension each, up to a total of 100 percent.

As part of the 1995 reform of the Italian social security system (Law 335/95), the survivor insurance scheme moved from universal to means tested. The reform, which was passed on August 8, 1995, affected all benefit payments starting on or after September 1, 1995 whose beneficiary is a spouse with no dependent children. As illustrated in the second column of Table 1, the new-regime replacement rate for surviving spouses with no dependent children nor grandchildren decreases sharply when the survivor's annual taxable income exceeds certain thresholds. Specifically, the replacement rate drops to 45 percent if the survivor's income is above three times the annual minimum pension, 36 percent if above four times the annual minimum pension and 30 percent if above five times the annual minimum pension.[15] The income measure used to determine the replacement rate is individual taxable income, inclusive of all forms of labor income from employment and self-employment, retirement income, pensions and retirement annuities, capital income and rental income, with the exclusion of the survivor pension. During the application stage and in each subsequent year, survivors are required to report their taxable income to the Social Security Administration.[16] If they fail to do so, they receive a pension equivalent to the minimum pension. The minimum pension is a minimum amount provided by the social security to pensioners whose pension benefit is below a subsistence threshold. The minimum pension level is set by law each

[13] Once the surviving spouse remarries, he/she receives a one-time lump-sum payment equivalent to two years of benefits.

[14] In those cases in which the deceased had not yet retired at the time of death, the survivor benefit is based on the pension that he or she would have been entitled to at the time of death, as determined by pension contributions paid up to that date. In case the deceased was on disability insurance at the time of death, the survivor benefit is computed on the basis of the disability benefit.

[15] The 1995 reform left unchanged the replacement rates for all other categories of recipients, with the exception of single dependent children whose replacement rate increased from 70 to 80 percent.

[16] Reported income refers to the previous fiscal year.

year. Table A1 reports the nominal value of the minimum pension and of its multiples for the years from 1990 to 2017.

Figure 1 illustrates the replacement rate schedule for individual spouses in the old and new regime. The x-axis represents the surviving spouse's taxable income net of the survivor benefit, denoted by z, and the y-axis represents the survivor replacement rate $b = \frac{B}{P}$, where B is the survivor benefit amount and P the pension of the deceased spouse. The dashed line refers to the old regime, in which a flat replacement rate of 60 percent applies uniformly irrespective of the level of z. The solid line refers instead to the new regime, whereby survivors with taxable income in the second, third and fourth income brackets are subject to reduced replacement rates. Denoting by j the income bracket, where $j = \{1, 2, 3, 4\}$, I_j indicates the taxable income threshold between bracket j and $j + 1$.[17] Importantly, the replacement rate schedule is kinked and not notched. This feature stems from a provision of the law preventing that the sum of individual income and survivor benefit in a higher income bracket be lower than what would be obtained in a lower income bracket. Formally, the benefit formula in the old regime is $B_j^O = b_j^O \cdot P \; \forall \; j$, where $b_j^O = 0.6$. In the new regime, the benefit formula reads $B_j^N = \max\{b_j^N \cdot P \; , \; b_{j-1}^N \cdot P + I_{j-1} - z\} \; \forall \; z$ in bracket j, where $b_1^N = 0.6$, $b_2^N = 0.45$, $b_3^N = 0.36$ and $b_4^N = 0.3$.

Interaction with personal income taxation. The tax base for personal income taxation includes all forms of labor income from employment and self-employment, retirement income, pensions and retirement annuities, capital income and rental income, *and* the survivor benefit. Importantly, since survivors under both the old and new regimes are subject to the same personal income tax schedule, income taxes do not affect the income wedge between old- and new-regime survivors and can therefore be assumed away in the analytical framework.[18]

The 1995 pension reform. The 1995 reform of survivor benefits was part of a broader set of measures, known as the "Dini Reform", whose main objective was to improve the financial sustainability of the Italian social security system. While remaining pay-as-you-go, the new pension system moved from a defined-benefit to a notionally defined-contribution scheme, and introduced greater flexibility in the retirement age. The reform initiated a progressive transition to the new system: workers with at least 18 years of contributions as of December 31, 1995 remained under the old defined-benefit system; workers with less than 18 years of contributions would be subject to a pro-rata system, with pension benefits computed using the defined-benefit formula up to the end of 1995 and the notionally defined-contribution formula starting from January 1, 1996; workers entering the labor market on or after January 1, 1996 would be fully subject to the new notionally-defined contribution system. In spite of the close timing of the pension and survivor insurance reforms, the former is unlikely to have any confounding effect on the identification of the causal

[17] More precisely, I_1 is equivalent to three times the minimum pension, I_2 four times the minimum pension and I_3 five times the minimum pension.

[18] Personal income tax brackets do not coincide with the income thresholds relevant to the computation of the survivor benefit in any of the years in the analysis.

impact of the latter, since the threshold dates of the two reforms are different. In Section 3.2, I will provide evidence that the effect of the pension reform is smooth at the September 1, 1995 cutoff.

2.2 Expected Effects of the 1995 Reform of Survivor Insurance

As illustrated in Section 2.1, the 1995 reform of survivor insurance generated a substantial change in the benefit schedule of surviving spouses without dependent children nor grandchildren. In this section, I describe the impact of the reform on the budget constraint of those spouses and its expected effects on their labor supply decisions.

Figure 2 illustrates the effect of the reform on the survivor's budget set in the (z, c) plane, where z indicates taxable income net of the survivor benefit and c denotes disposable income. Specifically, $c = z + B(P, b(z)) - T(z + B(P, b(z)))$, where $B(\cdot)$ is the survivor benefit, which is a function of the pension of the deceased P and of the replacement rate $b(z)$; $T(\cdot)$ is a tax function representing personal income taxes payable on taxable income including the survivor benefit $(z + B(\cdot))$. The dashed line represents the individual budget constraint under the old regime, while the solid line the individual budget constraint under the new regime. The vertical bars indicate the income brackets relevant to the determination of the benefit replacement rate in the new regime. Without any loss of generality, the plotted budget is constructed using the mean value of P, the income thresholds and the personal income tax parameters in effect at the time of the 1995 reform.[19] Individual utility increases with disposable income c, since disposable income provides consumption, and decreases with taxable income z, since it is costly to gain income.

In a static framework, survivors with taxable income above $z > I'_3$ experience a pure negative income effect with no change in the net-of-tax rate. Under the standard assumption that leisure is a normal good, individuals should respond to the negative income shock by increasing labor supply and hence taxable income. The same is true for individuals with taxable income in the ranges $[I'_1, I_2]$ and $[I'_2, I_3]$. Individuals with income $z \in [I_j, I'_j]$ for $j = 1, 2, 3$ experience both a negative income effect and an increase in the marginal tax rate on taxable income.[20] By reducing the net reward from additional work, the reform creates substitution incentives to reduce labor supply and taxable income for these individuals. In particular, given that the marginal tax rate is effectively equal to 100 percent for $z \in [I_j, I'_j]$, it is suboptimal for individuals to locate in this range. Assuming that the income-generating ability distribution is smooth, we should expect all treated individuals who would counterfactually locate in $[I_j, I'_j]$ to bunch at the convex kink I_j. However, since the range of taxable incomes over which the reform creates substitution incentives is narrow, one might expect negative labor supply responses to be limited. Conversely, positive labor-supply responses to the pure income effect are expected to arise over most of the taxable income distribution. The reform

[19] It is apparent from the graph that, by affecting individuals under both regimes in the same way, personal income taxation does not add to the wedge between the old and new regime benefit schedules.

[20] Formally, $I' = (b^N_{j-1} - b^N_j)P + I_{j-1}$.

does not affect individuals with income $z < I$. Thus, we do not expect to observe any changes for these individuals.

From a dynamic perspective, individuals under new-regime rules face lower net returns from each additional year of work. This is illustrated in Figure A1, which shows the relationship between lifetime consumption and the number of years of work (out of the total number of available years). The dashed line represents the individual lifetime budget constraint under the old regime, while the solid line under the new regime. It is clear from the graph that the reform creates dynamic income effects and substitution incentives with opposite expected effects on labor supply: income effects play in the direction of increasing labor supply at the extensive margin, for instance through a delay of labor-market exit and retirement; substitution incentives have the opposite effect.[21] The fact that income and substitution incentives work in opposite directions implies ambiguous expected effects on labor force participation along both the entry and the exit margin. Hence, it is an empirical question whether dynamic income or substitution effects prevail in the context of analysis.

3 Data and Empirical Strategy

3.1 Data

I use novel, confidential administrative data from the Italian Social Security (INPS) on the universe of survivor benefits in Italy. The survivor insurance archive comprises all survivor insurance benefits paid out to survivors of deceased retirees, disability insurance recipients and workers in the private sector, with starting date between January 1, 1990 and December 1, 2000. The archive includes detailed annual benefit information for each individual beneficiary within the household. Available information includes the start and end dates of the benefit, the pension of the deceased, the amount of the benefit before and after means testing, the number of beneficiaries in the household and their relationship to the deceased, the survivor's taxable income used to determine the replacement rate and the reason for benefit entitlement loss in case of benefit exhaustion.[22]

The survivor benefit archive can be linked to individual survivors' contributory histories that span from as early as 1900 up to 2017. The contributory archives provide detailed information on the entire working history of individuals, including both employment spells and spells related to in-work and out-of work social insurance, such as parental and family leave, sick leave and unemployment insurance. Information is also available on the duration of each spell and on earnings in each employment spell. The sample covers all employees of the private and public sector, as well as self-employed workers, independent contractors and professionals. For the subgroup of private-sector employees, I link the contributory records to the UNIEMENS file, which gives information on the

[21] The discussion rests implicitly on the assumption that leisure is a normal good.

[22] The data do not report the cause of death.

type of contract held by the worker (i.e. whether full-time or part-time), a unique identifier of the firm, the 5-digit industry code and the province in which he or she works in each year from 1983 to 2017. Finally, the data can be linked to the demographic archive, which provides information on gender, municipality of birth, birth date, retirement date and death date.

Combining the survivor benefit, contributory history and demographic archives, I build up a panel of individual working and benefit histories of survivor benefit recipients at annual frequency. The final dataset is a balanced panel of approximately 95,000 survivors spanning from six years before to 15 years after the spouse's death.[23] The sample comprises all surviving spouses aged 55 and under and not yet retired at the time of their spouse's death, and whose benefit started between September 1, 1993 and August 1, 1997.[24] Information on the number of formally dependent children and grandchildren is included in the data.

The first two columns in Table A2 report the mean and standard deviation of a set of individual characteristics for the main sample. The sample is predominantly female (90 percent) and the average age in $t = 0$ is 46.9 years. At the time of their spouse's death, 45 percent have dependent children, aged 13 years on average, and 40 percent are employed. Average annual labor earnings (unconditional on employment) are €6,200.[25] The average monthly survivor benefit in $t = 0$ amounts to €690, which translates into an average annual benefit of €9,700.[26] The table also reports separate summary statistics for surviving spouses whose benefit started before ("control" group) and after ("treatment" group) September 1, 1995.

3.2 Empirical Strategy and Identification Checks

The 1995 reform naturally defines a treatment and a control group as a function of the spouse's death date: benefits starting before September 1, 1995 fall under the universal scheme (henceforth, "control" group), while benefits starting on or after that date under the means-tested scheme ("treatment" group). In this way, the reform introduces a new, less generous benefit schedule that will coexist parallel to the old one until all old-regime benefits will have been exhausted. Such quasi-experimental variation allows to estimate the causal effect of unearned income on individual labor supply, earnings and total income, by comparing otherwise identical individuals subject to

[23] The balanced panel is conditional on the surviving spouse being alive in the 15 years after the spouse's death, and unconditional on employment and remarriage. When considering a balanced sample of survivors unconditional on survival, the survival rate 15 years after the spouse's death is not discontinuous at the cutoff. Similarly, there is no discontinuity in the survival rate 19 years after the spouse's death in the balanced sample used in the analysis. The remarriage rate – measured 15 years after the spouse's death – is approximately 5.6 percent. Remarriage occurs on average 10 years after the former spouse's death. There is no statistically significant discontinuity in the remarriage rate 15 years after the spouse's death nor in the time to remarriage.

[24] The choice of restricting the sample to spouses experiencing the shock at or before age 55 is motivated by the fact that I analyze long-run labor supply responses up to 15 years after the spouse's death. The modal age of retirement in the data is 60 and retirement can be considered an absorbing state in the Italian context.

[25] All monetary quantities are expressed in 2010 prices.

[26] The annual benefit is equivalent to 14 monthly instalments. Survivors receive twice the monthly benefit amount in July and December each year.

exogenously different benefit schedules for the rest of their lives. The ability to estimate labor supply and total income responses from long-run variation in unearned income is an important feature of this research setting, because it allows to obtain estimates likely not attenuated by short-run optimization frictions and therefore closer to the structural parameter of interest. A second important feature of this research setting is that, by comparing treated and control individuals similarly affected by a spouse's death, it implicitly controls for state dependent preferences and potential anticipation effects.

The structural model that describes the causal relationship of interest is:

$$Y_{it} = \alpha + \beta \cdot B_{it} + X_{it}' \cdot \gamma + \varepsilon_{it} \tag{1}$$

where Y_{it} is the outcome of interest Y for individual i; t indicates event-time years after the death event; B_{it} is the amount of the survivor benefit received by i in t; and X_{it} represents a vector of controls. In this model, the parameter of interest is β, which captures the causal effect of unearned income B_{it} on the outcome Y_{it}. For $Y = z$, where z is taxable income, β identifies the marginal propensity to earn out of unearned income MPE $= \frac{dz}{dB}$. Given the potential endogeneity of B, I exploit exogenous variation in the benefit replacement rate due to the 1995 reform and use the September 1995 cutoff as an instrument for B.

The policy change lends itself to the implementation of a regression discontinuity (RD) design in the benefit start date around the September 1, 1995 cutoff.[27] The empirical strategy for this RD design is illustrated in Figure 3. The x-axis represents the month-year of benefit start, with the vertical line indicating the September 1995 threshold. The graph shows the average replacement rate by month-of-benefit-start bin for surviving spouses with taxable income in the second, third and fourth income brackets in the first year of benefit receipt. The graph provides compelling evidence of the reform implementation: all benefit payments with start date prior to the cutoff had a replacement rate of 0.6; benefit payments with start date immediately after the cutoff have a substantially lower replacement rate. At the threshold, the replacement rate drops by 14 percentage points to 0.44. Because of this sharp and exogenous discontinuity in the benefit replacement rate, I can estimate the structural form in model 1 using the September 1995 threshold as an instrument for B.

The first stage equation is estimated using a parametric RD specification of the following form:

$$B_{it} = \alpha_0 + \beta_0 \cdot \mathbb{I}[\tau_i \geq 0] + \sum_{k=1}^{K} \alpha_k \cdot \tau_i^k + \sum_{k=1}^{K} \beta_k \cdot \tau_i^k \cdot \mathbb{I}[\tau_i \geq 0] + X_{it}' \cdot \delta + \mu_{it} \tag{2}$$

where τ_i is the benefit start date for survivor i normalized so that $\tau = 0$ at the cutoff date

[27] Note that using the benefit start date as running variable is essentially equivalent to using the deceased's death date, since benefits start on the first day of the month immediately after the death.

of September 1, 1995, and all other variables are defined as before. The coefficient of interest capturing the effect of the discontinuity at $\tau = 0$ is β_0. Polynomials in τ of order K are included to control in a flexible way for the effect of benefit start date τ on the outcome variable. The reduced-form equation is equivalent to equation 2 with Y_{it} as outcome variable:

$$Y_{it} = \theta_0 + \eta_0 \cdot \mathbb{I}[\tau_i \geq 0] + \sum_{k=1}^{K} \theta_k \cdot \tau_i^k + \sum_{k=1}^{K} \eta_k \cdot \tau_i^k \cdot \mathbb{I}[\tau_i \geq 0] + X'_{it} \cdot \lambda + \nu_{it} \tag{3}$$

The key assumption for identification in an RD design is that treatment is as good as randomly assigned in a neighborhood of the cutoff and that counterfactual outcomes are smooth at the cutoff. This identification requirement would be invalidated if there were some strategic manipulation around the threshold in anticipation or in response to the policy change. Figure A2 plots the probability density function of benefit recipients by month-year of benefit start for the entire sample (Panel A) and for the subgroup of individuals with taxable income in the second or higher income bracket at time $t = 0$ (Panel B). There is no visible discontinuity in the density around the threshold in none of the two plots. The McCrary test statistics reported on each panel do not reject the null hypothesis of no discontinuity at the threshold. On top of providing supporting evidence for the identifying assumption, these results also show that the reform had no effect on survivor benefit take-up.

The RD identifying assumption implies that individuals around the cutoff are comparable in their observable and unobservable characteristics. I perform a covariate balancing test using parametric and non-parametric RD specifications. As reported in Table A3, covariates are balanced under both the linear and quadratic parametric specifications, and the local linear regression specification. It is worth emphasizing that the proportion of individuals subject to a defined-benefit pension regime is smooth at the cutoff, indicating that the 1995 reform is not a confounder in the estimation of the causal effect of the 1995 survivor benefit reform. Based on the balancing test results, I select the parametric RD with a second-order polynomial fit and with covariates as my preferred specification. Output tables also report estimates for the parametric linear specification. Estimates are based on month-of-benefit-start bins and a symmetric bandwidth of 24 months. Figures A3, A4 and A5 report parametric quadratic RD estimates of the main outcomes of interest for a set of different bandwidths.

As illustrated in Section 2.2, the reform only affects individuals with incomes in the second or higher brackets. In order to focus on the subgroup of individuals that are more likely to be affected by the reform, ideally I would need a measure of the counterfactual income bracket in which treated individuals would locate absent the reform. On the one hand, the observed income bracket in $t = 0$ (i.e. in the first year in which it is recorded in the data) may be sufficiently exogenous to labor supply choices in response to the reform in a neighborhood of the 1995 cutoff.

However, it is unlikely to be a good proxy for the long-run income bracket in both the treatment and control group, due to idiosyncratic income shocks correlated with the spouse's death. On the other hand, the observed longer-run income bracket is endogenous to the policy change for individuals in the treatment group. To overcome these limitations, I employ statistical-learning techniques and develop an empirical model to predict the long-run counterfactual income bracket in the treatment group using observations in the control group. Having randomly selected ten percent of individuals in the control group (training sample), I predict their income bracket at time $t = 10$ using a rich set of pre-determined demographic characteristics and variables related to their working history prior to widowhood. Among this rich set of covariates, I select a parsimonious subset of most relevant predictors using a Lasso estimator. Finally, I apply the coefficients of the prediction model – an OLS regression of income bracket in $t = 10$ on the selected covariates – to observations in the treatment group and predict their long-run counterfactual income bracket. This procedure allows to define a group of individuals, in both treatment and control groups, with predicted income in the second or higher income brackets. I conduct the empirical analysis on this sample, since it is the one likely most affected by the reform.

Summary statistics for the sample with predicted income in the second or higher income bracket are reported in Table A4, both for the full sample and for the treatment and control groups separately. As one would expect, the sample of "affected" survivors has larger average labor income (€24,200) and a much higher labor force participation rate (0.96 in $t = -1$) as compared to the full sample. The sample is still predominantly – albeit less prominently – female (64 percent) and slightly younger than the main sample (43.5 years old on average). The average monthly benefit is also higher, consistent with the notion of assortative mating.

As discussed in Section 2.2, the policy change creates a large income effect for all individuals with taxable income in the second or higher income brackets. At the same time, substitution incentives may arise as a result of marginal tax rate changes over small portions of the taxable income distribution. In order to identify the marginal propensity to earn out of unearned income – the income effect –, I first estimate the effect of the benefit on taxable income using the IV-RD strategy described above. Formally, if substitution incentives matter and the compensated elasticity is greater than zero, then the IV-RD estimate of β provides a lower bound of the true income effect.[28] Secondly, I provide evidence consistent with substitution incentives having a limited role and conclude that the estimated $\hat{\beta}$ coefficient from model 1 indeed provides a measure of the structural marginal propensity to earn out of unearned income.

[28]From the Slutsky equation, the total (uncompensated) labor supply response to a benefit change is the sum of a positive compensated effect and a negative income effect ($= dz/dB$).

4 Results

4.1 First Stage

Based on the empirical strategy outlined in the previous section, I use having benefit start date on or after September 1, 1995 as an instrument for the amount of survivor benefit received. Figure 4 shows the first-stage effect of benefit start date on expected lifetime benefit in $t = 0$. The lifetime benefit is computed by multiplying the annual benefit in $t = 0$ by life expectancy at time $t = 0$. Life expectancy tables are obtained from the Italian Statistical Institute (ISTAT) and are split by gender, age, calendar year and region of residence. The discontinuity in lifetime benefits is estimated to be approximately €100,000 and is equivalent to a 31 percent drop when compared the mean in the control group.[29] The RD estimate is large and highly statistically significant, indicating that having benefit start date on or after the cutoff date indeed translates into a substantial reduction in benefits.

Table 2 reports estimates of the coefficient β_0 from equation 2 using either the annual benefit in $t = 0$ or the expected lifetime benefit in $t = 0$ as outcome variable. Estimates in the top panel are based on the sample of individuals with predicted second or higher income bracket, while those in the bottom panel on the full sample of surviving spouses. According to the estimates reported in column (4) of the top panel, individuals with benefit start date after the cutoff receive annual benefits in $t = 0$ that are on average €2140 or 25.2 percent lower than those received by otherwise identical individuals in the control group. The second row of the top panel of Table 2 reports the RD estimate of the effect of the reform on survivor's lifetime benefit, which was discussed in the previous paragraph.

The bottom panel of Table 2 shows similar estimates for the full sample of surviving spouses. Consistent with part of this sample having taxable income $z < I_1$ and hence not being affected by the reform, the estimated effect is smaller than the one reported in the top panel. Specifically, the annual benefit drop in $t = 0$ is of €600 (7.1 percent of the mean in the control group) and the expected lifetime benefit drop as of $t = 0$ is of €23,600 (3.9 percent of the mean in the control group). These results confirm that the prediction model described in Section 3.2 well identifies a subgroup of individuals most heavily affected by the reform.

4.2 Effect of the Benefit on Taxable and Disposable Income

In this section, I estimate the long-run effect of the benefit on taxable income and disposable income. I first provide reduced-form evidence of the effect of the 1995 reform on the outcomes of interest. I then complement the reduced-form evidence with structural-form estimates of the marginal propensity to earn out of unearned income from IV estimation of model 1. In the analysis,

[29] The mean in the control group is measured as the average of the outcome variable for surviving spouses with benefit start date between May and August 1995.

I follow an extensive literature that uses taxable income as an all-encompassing measure of labor and other behavioral margins of response to changes in the tax and benefit system (Feldstein [1995]; Saez, Slemrod and Giertz [2012]). Of course, there could be additional sources of income that are unobserved in the data, for instance undeclared income and income support from relatives. If anything, the effect that I estimate should be a lower bound of the true effect if unobserved income plays a similar role in response to the benefit reduction.

Panel A of Figure 5 reports the reduced-form RD effect of the reform on the average annual benefit over the fifteen years after the spouse's death. The graph is constructed pooling event time years from $t = 0$ to $t = 15$. Individuals in the treatment group receive approximately €2,000 less in survivor benefits on average each year – an amount equivalent to 20.7 percent of the mean in the control group. At the same time, their reported taxable income (excluding the survivor benefit) is on average €2,300 or 15.8 percent larger than that in the control group over the same time period (Panel B). The sum of these two roughly equally sized but opposite signed effects implies that the net reduced-form effect on disposable income is quantitatively small and precisely equivalent to €300 or 1.5 percent over the mean in the control group (Panel C). Regression estimates of the reduced-form model for average annual benefit, taxable income and disposable income are reported in Table 3. The reduced-form results indicate that individuals fully offset the benefit loss with a tantamount increase in taxable income in the fifteen years following their spouse's death. This is also confirmed by the IV-RD estimates of the β coefficient of model 1 reported in Table 4. According to the estimates in column (3), the marginal propensity to earn out of unearned income is equal to -1, i.e. a €1 decrease in average annual survivor benefits is associated with a €1 increase in taxable income.[30] Such estimated effect is large and provides a lower bound of the true income effect for a positive compensated elasticity. The 95 percent confidence interval around the estimate allows to reject parameter estimates lower than 0.4 in absolute value, which is itself at least twice as large as most existing estimates in the literature. Consistent with the reduced-form evidence, the net effect on disposable income is essentially zero (column (4) of Table 4).

Rescaling the estimated income effect by the ratio of the benefit to taxable income provides a measure of the income elasticity, i.e. the percent change in taxable income for a one percent change in the benefit. Since the ratio B/z is approximately 0.6 in a left neighborhood of the threshold, it follows that the income elasticity is approximately -0.6. Based on a 95 percent confidence interval, I can reject elasticities lower than 0.25 in absolute value.

Mean income effects mask substantial heterogeneity across subgroups. As shown in Table A5, the income effect is one order of magnitude larger, in absolute value, for women than for men. Such heterogeneity in income effects likely reflects heterogeneity in the severity of the income shock

[30] The estimate of the income effect is robust to different parametric specifications. The linear and quadratic specifications are statistically similar (Table 4). The parametric quadratic estimates are stable across bandwidths, with the exception of the 18-month bandwidth (Figure A3).

across gender. Since women are predominantly secondary earners in the household, the benefit drop that female survivors face as a consequence of the reform is, on average, larger than that of male survivors. This is confirmed by the results in Table A5, which show that female survivors lose approximately €2000 per year, while male survivors only €700. Moreover – as secondary earners – female survivors tend to have lower taxable incomes than male survivors, as illustrated in Panel A of Figure A6. The graph plots the empirical distribution of the predicted taxable income bracket by gender and shows that, indeed, female survivors tend to have lower predicted taxable incomes then male survivors. The greater severity of the income shock faced by female survivors is a factor that can help explaining the substantially larger income response among this group. Turning to heterogeneity by age at the time of the spouse's death, the income effect is monotonically decreasing over the life cycle. This pattern may be explained by the fact that the ability to increase earned income declines at older ages, due to both higher disutility from work and slimmer labor market opportunities. The availability of sources of self-insurance other than labor supply, such as savings and children's labor supply, may also be greater at older ages, thus limiting the need to adjust taxable income.[31]

Validating the identification of the income effect. I now turn to investigating how important substitution incentives are in the context of analysis. Firstly, I show that the estimated income effect is robust to the exclusion of individuals with taxable income in a neighborhood of the convex kinks created by the reform. Table A6 reports the IV estimate of the marginal propensity to earn out of unearned income, based on the sample of individuals with predicted income in the second or higher income bracket, with the exclusion of individuals whose observed taxable income falls in the second or third income bracket. The IV-RD estimate of the income effect is in line with the one obtained in the main sample, though less precisely estimated due to smaller sample size.

Secondly, I take advantage of the discontinuities in the marginal tax rate introduced by the 1995 reform to infer the value of the compensated elasticity using a bunching estimator (Saez [2010]; Kleven [2016]). Let individual preferences be defined over disposable income (consumption) and taxable income (work effort). A utility function representing such preferences is $U = u(z - T(z), z/\theta)$, where $T(\cdot)$ is a tax function and θ is income-generating ability, distributed with probability density function $f(\theta)$. If $T(\cdot)$ is linear and $f(\theta)$ smooth, then the probability density function of z is also smooth. Figure A7 illustrates a theoretical density function of taxable income z. The dashed line illustrates the case of a smooth density function. By introducing discrete changes in the marginal tax rate, the reform creates three convex kinks in the budget constraint of treated individuals at $z = I_j$ for $j = 1, 2, 3$. Absent the kink (as under old-regime rules), individuals would locate smoothly along the old-regime budget set. Once introduced, the convex kink creates a disincentive

[31] As Panel B of Figure A6 shows, differences in the empirical distribution of predicted taxable income bracket across age groups are limited. For individuals in the 50-55 age group at the time of their spouse's death, the distribution has slightly more mass at the lower end of the distribution.

for individuals to locate in the range $[I_j, I'_j]$ (since the marginal unit of income is taxed away at a 100 percent tax rate over that range) and induces individuals who would counterfactually locate in that range to bunch at I_j. This behavior will give rise to excess bunching in the taxable income density function at the kink point and to a left-shift in the density above the kink, as illustrated by the solid line in Figure A7. Hence, the presence of bunching provides compelling evidence of taxable income responses to the marginal tax rate change. As shown in Saez [2010], the amount of excess bunching is proportional to the compensated elasticity of taxable income and can be used to identify such elasticity.

The 1995 reform introduced three convex kinks in the budget set of individuals in the treatment group (Figure 2). If substitution incentives are at play, we should observe bunching around kinks in the treatment group and a smooth density in the control group. Figure 6 plots the empirical distribution of taxable income pooling observations around the three convex kinks created by the reform and pooling all years from $t = 0$ to $t = 15$.[32] The vertical bar represents the location of the convex kink. Each dot refers to a €200 bin in the range $[-2,700; 2,700]$ centered around the kink. Black circles represent observations in the treatment group (kinked budget), while hollow circles observations in the control group (smooth budget). The empirical distributions of both groups appear rather similar throughout the range and equally smooth around the kink, in spite of the rather different incentives faced by the two groups at that point of the income distribution.

In principle, the absence of excess bunching is consistent with different theoretical interpretations: on the one hand, it is consistent with the compensated structural elasticity being small; on the other hand, it is also consistent with the compensated structural elasticity not being small, but the observed elasticity being attenuated by optimization frictions. Optimization frictions may come in the form of costs of adjusting labor supply, such as hour constraints, or in the form of imperfect information, inattention and inertia. Adjustment costs are believed to be of less importance for self-employed workers and to become more attenuated over time. Yet, even when splitting the sample between self-employed and wage earners – as illustrated in Panels A and B of Figure A8, for individuals in the treatment and the control group respectively –, there appears to be no visible difference in the empirical densities nor excess bunching at the kink for self-employed in the treatment group. This evidence is thus consistent with the fact that adjustment costs may not be responsible for the lack of excess bunching. Adjustment costs, imperfect information and inertia should all fade away in the long term. The graphs in Figures 6 and A8 are both constructed using observations for event-time years from $t = 0$ to $t = 15$ – a time span that should be sufficiently long for adjustment costs, information frictions and inertia to dissipate. Thus, the lack of bunching over such a long period of time seems unlikely to be due to these types of frictions.

Cognitive biases may make individuals misperceive the way in which the new-regime benefit

[32] Results do not change when replicating the analysis around each of the three kinks separately and for each of the event-time years separately.

schedule affects the budget constraint.[33] One such possibility is that the benefit schedule (and in turn the budget constraint) is understood as notched and not kinked. If this were the case, however, one should still expect to see excess bunching at the kinks, making this type of misperception unsuitable to explaining the lack of bunching. A type of cognitive bias consistent with the absence of bunching is "ironing", whereby individuals make decisions based on average rather than marginal tax rates and, therefore, do not react to the latter. Cognitive bias, inattention and low salience of the benefit schedule are all factors that could explain the absence of bunching. Whilst I cannot completely rule out their playing a role, nonetheless I can exclude that individuals are responding to static substitution incentives in the context that I study. Individuals may still be responding to dynamic substitution incentives, in which case the estimated $\hat{\beta}$ is a lower bound (in absolute value) of the structural income effect.

Comparison with existing quasi-experimental estimates. The taxable income response estimated in this paper is substantially larger than the existing empirical estimates of the marginal propensity to earn out of unearned income. As outlined in the introductory section, the literature on NIT experiments and lottery wins places a consensus estimate of the income effect at approximately -0.10 (Robins [1985], Hum and Simpson [1993], Ashenfelter and Plant [1990], Imbens, Rubin and Sacerdote [2001], Cesarini et al. [2017]). Yet, recent studies have found larger income effects on earnings in the context of disability insurance and social security wealth in the US. Deshpande [2016*b*] estimates a parental earnings response to the loss of Supplemental Security Income of approximately -1.4, while Gelber, Isen and Song [2016, 2017] estimate an upper bound of the elderly earnings response to the Social Security "Notch" of -0.6 for men and -0.89 for women.

The results in this paper are not necessarily inconsistent with the smaller estimates found in the literature. I here consider potential explanations for finding large income effects. Firstly, differences in the observable and unobservable characteristics – such as the degree of risk aversion – of the populations of analysis may explain differences in their marginal propensities to earn out of unearned income.[34] Secondly, responses may differ with respect to the type of income shock. In this regard, individuals may respond asymmetrically to gains and losses of unearned income (Deshpande [2016*b*]): responses to unearned income losses are likely to be larger than responses to unearned income gains whenever individuals are loss averse, have minimum income targets or sticky consumption habits (Kőszegi and Rabin [2006], Chetty and Szeidl [2007, 2016]).[35] The degree to which individuals are *ex-ante* insured against the income shock is also a factor that can influence

[33] This is what Liebman and Zeckhauser [2004] call "schmeduling".

[34] As shown in Chetty [2004], the coefficient of risk aversion is directly related to the size of the income effect on labor supply. *Ceteris paribus*, large income effects are evidence that utility over consumption is highly curved. Intuitively, if an individual increases labor supply sharply in response to a given drop in unearned income, it must mean that the marginal utility of consumption increases quickly when income falls, meaning that the individual is highly risk averse.

[35] Whilst individuals in the new regime never got the higher, old-regime benefit level, it is still the case that they may have expected higher benefits, especially given that the reform was little anticipated.

the magnitude of the income effect. Finally, individuals may behave differently with respect to different types of income and, consequently, have different marginal propensities to consume or earn out of different sources of unearned income (e.g. lottery wins as opposed to welfare transfers). This is what Thaler [1990] refers to as (absence of) fungibility. Given the available data, I have limited ability to probe these explanations.

Compatibility with macro elasticities of labor supply. I also examine whether the micro elasticity that I estimate is consistent with macro elasticities of labor supply. It has long been recognized that estimates of steady-state macro elasticities diverge from micro ones. Specifically, macroeconomic models of cross-country variations in hours worked imply elasticities that are much larger than those estimated using sources of identifying variation at the micro level (Chetty et al. [2013]). The literature has identified two main factors that can reconcile the macro and micro evidence on the elasticity of labor supply: optimization frictions (Chetty [2012]) and the indivisibility of labor (Rogerson [1988], Ljungqvist and Sargent [2007], Rogerson and Wallenius [2009]). Specifically, optimization frictions are likely responsible for the substantial attenuation of micro elasticities. The small and short-run policy variation that is typically exploited to identify micro elasticities is unlikely to generate large labor supply responses precisely due to optimization frictions such as adjustment costs. On the other hand, labor supply indivisibility – whereby agents face fixed labor-market entry costs or intensive-margin rigidities – is a feature of several macroeconomic models that reproduce large labor supply elasticities, and show that both intensive and extensive margins of labor supply are important to describe hour fluctuations.

Using data for OECD countries from 1985 to 2015, I run a simple regression of the logarithm of hours of work per person on the logarithm of GDP per hour, controlling for country and calendar year fixed effects.[36] Figure A9 reports a binned scatter plot of the regression of interest. The estimated elasticity of hours worked to GDP per hour is -0.56.[37,38] The latter is statistically compatible with my micro estimate, and with the steady-state frictionless hour elasticity identified in Chetty [2012]. Overall, this result suggests that the long-run identifying variation exploited in this paper can indeed prove useful in delivering a parameter estimate not attenuated by short-run optimization frictions.

4.3 Labor Supply Responses

The large taxable income response to the benefit cut prompts several questions. In this and the following section, I probe the mechanisms behind the income response. I first investigate

[36]The countries included in the sample are Australia, Belgium, Canada, Germany, Denmark, Finland, France, the United Kingdom, Italy, Japan, the Netherlands and the United States.

[37]The robust standard error of the coefficient estimate is 0.065. The estimated macro elasticity is likely to conflate both substitution and income effects, and thus likely to provide a lower bound of the income elasticity itself.

[38]I also run an alternative specification in which I regress the logarithm of hours of work per person on the logarithm of total factor productivity, controlling for country and calendar year fixed effects. The estimated elasticity is -0.30 (robust standard error 0.077). Results are available upon request.

the anatomy and dynamic of the labor supply response, and then examine effects on program substitution.

Anatomy of labor supply responses. The effect on earned income can be decomposed along three margins: labor force participation, hours of work and the wage rate. Figure 7 shows the reduced-form effect of the reform on labor force participation, pooling event-time years from $t = 0$ to $t = 15$. Labor force participation is 7.6 percentage points higher to the right of the cutoff. This effect is equivalent to a 12.7 percent increase over the mean in the control group. The IV-RD estimate reported in Table 6 indicates that an average annual €1,000 decrease in the survivor benefit leads to a 4 percentage point increase in labor force participation (6.6 percent over the mean in the control group). Another measure of the extensive margin of labor supply is the number of years of cumulated experience in the 15 years after the spouse's death. As reported in Table 5, cumulated experience in $t = 15$ is approximately 1.1 year higher for individuals in the treatment group.

The observed participation response could be due to either increased entry in the labor market or delayed exit from the labor market. Figure 8 shows a decomposition of the increase in cumulated experience over the 15 years after the death along the entry and exit margin. Specifically, the first bar to the left reports the average increase in cumulated work experience over those 15 years (equivalent to 1.1 years). The remaining three bars decompose such effect into increased entry (second bar from the left) and delayed exit, distinguishing between delayed exit in the form of delayed non-employment (third bar from the left) and delayed retirement (fourth bar from the left). I measure the entry margin by looking at the participation response of individuals who were not working in $t = -1$ and weight the estimate by the share of such individuals in the full sample. I measure delayed non-employment and delayed retirement as the reduced-form effect on the number of years not in employment (excluding retirement) and the number of years in retirement over the 15 years after the spouse's death (weighted by the share of individuals who were working in the year before their spouse's death). The observed participation response is driven both by increased entry and postponed retirement. In particular, delayed retirement appears to be the main driver of the labor supply response. The delay-effect on retirement is also confirmed by the reduced-form estimates in Table 5 and the IV estimates in Table 6: according to the latter, an average annual €1,000 decrease in the survivor benefit leads to a 10 percentage point decrease in the retirement rate in $t = 15$, representing a 19 percent decrease relative to the mean in the control group.[39]

Being an average effect, the result in Figure 8 is largely driven by the age composition of the

[39] I probe the heterogeneity of the retirement rate response between individuals employed in the private and the public sector. To this end, I focus on individuals who were working in the years prior to their spouse's death and construct an indicator variable for being employed in the public or private sector based on their employment history in $t < 0$. I find that the retirement rate response is entirely driven by individuals employed in the private sector. This is consistent with the notion that public-sector employees have limited ability of adjusting the retirement margin. Results are available upon request.

sample and masks important responses along the entry margin by individuals at younger ages. To shed light on this point, Figure 9 outlines the profile of the participation response by age in $t = 0$. The shaded area shows the 95 percent confidence interval of the reduced-form RD estimate for labor force participation for individuals in different age groups, irrespective of their employment status in $t = -1$. Black circles report the same coefficient for individuals who were not working in $t = -1$, and hollow circles for individuals who were working in $t = -1$. The mean effect on participation (represented by the shaded area) is therefore a weighted average of an entry effect (represented by the black circles) and a delayed exit effect (represented by the hollow circles). Comparing the magnitude of the labor supply response in the full sample with that in the subgroup that was not working in $t = -1$, one can infer that the entirety of the labor force participation response of individuals in younger age groups (20-40 and 41-50 years old) is in the form of labor market entry. Conversely, the participation response comes predominantly from delayed labor market exit for individuals in older age groups (51-55 years old).

Having established substantial extensive-margin responses, I now move to investigating intensive-margin and wage rate responses. Since the data provide information on days worked but not hours worked, I use days worked as a measure of the intensive margin of employment. The wage rate is defined as earnings per day worked conditional on employment. When analyzing outcomes conditional on employment, I control for potential endogenous selection into employment by including the number of years of work experience in $t = 0$ among the individual-level covariates (Schmieder, von Wachter and Bender [2016]). Albeit imperfectly, this allows to isolate the effect of the reform on hours worked and the wage rate from that of compositional changes of the workforce due to extensive-margin responses to the reform itself. The IV estimates in Table 6 show a statistically significant, yet mild effect of the benefit on days worked and on the wage rate: a €1,000 benefit drop is associated with a decrease in days worked of 2.6 days per annum and a decrease in the daily wage of €2.2 on average. The results suggest that surviving spouses may be moving to part-time, slightly lower paid jobs. However, both effects are especially small, both in absolute terms and in percent of the mean in the control group (0.7 and 2.3 percent respectively).

I further investigate the anatomy of the labor supply response by looking at the conditional probability of holding a full-time job, and of changing firm, industry and province of work.[40] I observe these outcomes only for the subsample of individuals with a job in the private sector. According to both the reduced-form and IV estimates reported in Tables 5 and 6 respectively, no statistically significant effect can be detected on the probability of holding a full-time job nor of changing firm, industry or province.

Dynamic of labor supply responses. The participation response estimated pooling all event-time years masks interesting dynamics. Figure 10 uncovers the evolution of the participation

[40] I look at transitions across 3-digit industries.

response over event-time years from $t = -6$ to $t = 15$.[41] Black circles report the reduced-form RD estimate at each event-time year in percent of the mean in the control group. Vertical capped bars indicate 95 percent confidence intervals. Consistent with the absence of anticipation of the reform and/or manipulation around the threshold, there is no discontinuity in the participation rate in the years before the spouse's death. The participation response unfolds progressively over time, being small and statistically insignificant in the first two years after the shock and then growing quite steadily over time, from 7 percent in $t \in [2; 3]$ to 18 percent in $t \in [14; 15]$. Analogous to Figure 10, Figure A10 reports the evolution of the labor force participation response in levels. The effect is muted up to event time $t = 1$, grows to a statistically significant 6.4 percentage point difference at event time $t \in [2; 3]$ and then stabilizes at an approximately 7 percentage point difference in subsequent event-time years. The evolution of the labor supply response is consistent with the notion that optimization frictions, such as adjustment costs or attention costs, attenuate responses in the short-run and fade away over time, allowing to uncover frictionless structural responses only in the medium-long run.

I also examine the dynamic of hours worked and the wage rate. Figures 11 and 12 report the reduced-form effect on the number of days worked and the daily wage conditional on employment at each event-time year. The dynamic of both variables is essentially flat throughout the 15 years following the spouse's death, indicating no intensive-margin adjustments and suggesting that work experience, human capital accumulation and effort have a limited role in the context under study.

Heterogeneity of labor supply responses. There is substantial heterogeneity in participation responses by gender. As reported in Table A5, the female participation rate increases on average by 10 percentage points (15.8 percent of the mean in the control group) in the 15 years after the spouses death, while the male participation rate by 4.5 percentage points (8.1 percent of the mean in the control group). This difference is consistent with what found for the income effect in Section 4.2. A stark gender differential also emerges when examining the dynamic pattern of labor force participation over event-time years, as shown in Panel A of Figure A11: the dynamic of the female subgroup displays a spectacular growth over event-time years, while that of the male subgroup is rather steady. There is no statistically significant difference by gender in the intensive margin response, as measured by the number of days worked (Table A5). As for the wage rate, male survivors experience a statistically significant decrease in the daily wage, conditional on employment, of approximately €5.85, equivalent to 7 percent of the mean in the control group. No significant effect is detected for female survivors.

The dynamic of the participation response by age at the time of the spouse's death confirms the role of retirement as a margin of adjustment: the labor force participation response of individuals in the 51-55 age group increases sharply over event-time years 2 to 7, and then drops to zero in subsequent years. This is consistent with a delay in retirement occurring in the late fifties and early

[41] To improve the precision of the estimates, I estimate dynamic effects pooling event-time years into biennia.

sixties. Interestingly, an analogous increase in labor force participation emerges around event-time years 12 to 15 for individuals in the 41-50 age group. As reported in Table A5, the age profiles of the intensive margin response and the wage rate response, conditional on employment, are essentially flat and statistically insignificant, except for a small, positive and statistically significant effect on the wage rate for individuals in the 20-40 age group, which are found to increase by approximately 1.8 percent.

4.4 Program Substitution

The reduction in survivor insurance generosity may induce survivors to take up more of other social insurance and social assistance programs in order to increase their disposable income. This is what previous studies have defined *program substitution* (Inderbitzin, Staubli and Zweimüller [2016]).[42]

Social insurance take-up. The data provide information on the take-up of work-related social insurance benefits, such as paid family leave, paid sick leave and unemployment benefits. Paid family leave includes both maternity/paternity leave and parental leave provided to individuals who need to take time off work to care for an ill child or relative. Paid sick leave is a benefit paid to workers who need to take time off work while sick. Unemployment benefits are publicly-provided benefits granted to laid-off private-sector employees. Since the take-up of these social insurance benefits is conditional on being employed at the time of take-up or on having been employed in the previous months, I restrict the sample to surviving spouses in employment in t or $t-1$. Moreover, in order to control for potential endogenous selection into program eligibility due to the conditioning on employment status, I control for the number of years of working experience in $t = 0$. According to the IV estimates in Table 6, every €1,000 decrease in benefits increases the probability of taking up paid family leave by 0.3 percentage points, which represents 37.5 percent of the mean in the control group.[43] The increase in paid family leave suggests that surviving spouses who increase their labor supply may be doing so under substantial work-time constraints due to family and care duties. While no significant effect can be detected on the probability of taking up paid sick leave, unemployment insurance take-up increases by 1.7 percentage points for every €1,000 decrease in benefits (a 100 percent increase over the mean in the control group). These results indicate that individuals in the new regime compensate for the less generous survivor benefits by increasing their take-up of alternative welfare programs. In this respect, the increase in unemployment insurance take up suggests that individuals may be willing to pay the cost of unemployment stigma to increase disposable income.

Children's dependency period. The 1995 reform reduced the benefit replacement rate for surviving spouses with no dependent children, while leaving unchanged the replacement rate for

[42] In principle, less generous benefits may also affect the take-up of survivor insurance itself. However, the results presented in Figure A2 allow to exclude any differential take-up around the threshold.

[43] Reduced-form estimates are reported in Table 7.

surviving spouses with one or more dependent children, who face a replacement rate of 80 percent and 100 percent respectively. Hence, the benefit drop experienced when children lose their dependency status is larger for surviving spouses with benefit start date on or after the September 1, 1995 threshold. This is confirmed by the results in Figure A12 and in the first row of Table 8. The latter reports the estimated effect of the reform on the benefit received by surviving spouses upon loss of children's dependency. Individuals in the treatment group suffer a €1,305 larger benefit loss than individuals in the control group. The level effect corresponds to 16.6 percent of the mean in the control group. It follows that, at the margin, one extra year with children as dependent is much more valuable for individuals in the treatment than the control group. Indeed, as shown in Figure 13, the number of years with dependent children is 1.2 years greater in households with benefit start date to the right of the cutoff. The IV-RD effect reported in Table 6 indicates that a €1,000 benefit drop increases the dependency period by 0.7 years – a 10.7 percent increase above the control mean.[44] To be classified as dependent, a child must be either aged under 18, or enrolled in high school and not working up to age 21, or enrolled at university and not working up to age 26. Thus, extending children's dependency period can be viewed as a costly action – namely paying enrolment fees – that surviving spouses undertake in order to increase disposable income.

5 Interpretation

5.1 External Validity and Policy Relevance

Whilst there is a selection issue of studying survivor benefit recipients, nonetheless there may be scope for generalizing the findings obtained in this context. Individuals in my sample are spouses – prevalently women – who become widow(er)s in their mid forties. Single-parenthood, and single-motherhood in particular, are among the states most at risk of income insecurity and single parents make up a large proportion of welfare recipients of programs such as Earned Income Tax Credit (EITC), Aid to Families with Dependent Children (AFDC) and Temporary Assistance to Needy Families (TANF). Hence, to the extent that widow(er)s in my sample can be representative of single parents in general, my findings can be relevant to a larger set of public policies in the US and in Europe.

To assess the validity of this hypothesis, I compare the characteristics of survivor benefit recipients in my sample with EITC recipients in the US, using data from the March Current Population

[44] A potential concern is that the estimated increase in the dependency period is spuriously driven by the fact that the cutoff date is in September – the month in which school years start and university enrolment takes place – and children who lost one of their parents in August may end up delaying their school or university enrolment by approximately one year. I test the validity of this alternative hypothesis by running placebo RD regressions around the September cutoff in the three years before and after 1995. Results are reported in Table A7. The estimates reveal a statistically significant positive effect only around the September 1995 cutoff. The estimated effect for September 1994 is statistically significant, but of negative sign. Overall, these results lend support to the idea that the observed increase in the dependency period is indeed a behavioral response to the incentives created by the 1995 reform.

Survey (CPS) in the years 1993 to 1997. Summary statistics for the sample of household heads receiving EITC are reported in Table A8, where column (1) refers to both married and single household heads and column (2) to single parents or single individuals. EITC recipients tend to be younger than individuals in my sample. Consequently, a higher fraction has dependent children and dependent children tend also to be younger. Apart from the age composition, labor force participation and taxable income are in line with those of benefit recipients in my sample.

Losing a spouse at a young age is a low-probability and relatively unpredictable event against which households are likely to be limitedly insured. In this respect, my estimates of the labor supply and income response are likely to provide an upper bound of what would be expected for "shocks" that are easier to anticipate and insure *ex ante*, such as own or spousal job loss.

Finally, the elasticity of labor supply may differ between marriage and widowhood. For instance, the lack of leisure complementarities between spouses and the desire to engage in working activities due to loneliness may make widow(er)s' labor supply more elastic. Conversely, the inability to share family-related duties with their partners may make widow(er)s' labor supply less elastic. To the extent that labor supply elasticities may differ across marital statuses, the effects estimated in this paper are likely to provide an upper or a lower bound for what would be expected for married individuals.

5.2 Implications for Theoretical Models of Labor Supply

In this section I examine how the findings in this paper connect with theories of labor supply. One simple way to theoretically rationalize the magnitude of the estimated income effect is to assume that individual preferences are quasi-linear in labor. Similar to the framework introduced in Section 4, suppose individual preferences are defined over consumption c (disposable income) and work effort $\frac{z}{\theta}$, where z is income from work and θ income generating ability. Individuals maximize a utility function $U(c, z)$ that is concave in consumption and linear in work effort

$$U(c, z) = u(c) - \frac{z}{\theta} \tag{4}$$

subject to the budget constraint $c = z + B$. The optimal levels of consumption and work (c^*, z^*) are such that $\partial c^*/\partial B = 0$ and $\partial z^*/\partial B = -1$.[45] In response to a drop in unearned income, income from work increases one-for-one and the level of consumption remains unchanged.

The "time-averaging" or "career-length" model proposed by Ljungqvist and Sargent [2007] is one example of a dynamic model that – in the reduced form – delivers predictions that are observationally equivalent to those of the above static model with quasi-linear preferences. Ljungqvist and Sargent [2007] construct a non-stochastic, continuous-time life-cycle model with time-separable

[45] The optimal levels of consumption and work are (implicitly) defined by $u'(c^*) = \frac{1}{\theta}$ and $z^* = \frac{1}{\theta} - B$.

preferences and labor supply indivisibility, in which a representative agent decides what fraction of her lifetime to devote to work.[46] A model of this type delivers a high labor supply elasticity at the extensive margin (i.e. in the number of years of work), which is observationally consistent with the finding in this paper that individuals lengthen their careers by delaying retirement in response to a negative income shock.

6 Normative Implications

A central result of this paper is that benefit losses trigger large labor supply and earned income responses. Why is the income effect that I estimate so large? On the one hand, if an individual increases labor supply sharply in response to a benefit drop, that lost income must be of high utility value. On the other hand, a substantial labor supply response may arise if labor-supply adjustment costs are low. Understanding which of these two alternative mechanisms – high utility value vs. low adjustment costs – prevails is important for welfare analysis.

To gain more formal intuition of the interplay between utility value and adjustment costs, assume individuals choose their consumption and labor force participation status to maximize a utility function that satisfies

$$u(c) - \mathbf{I}\{l = 1\} \cdot \phi \tag{5}$$

where $u(\cdot)$ is a concave function, c is consumption, $l \in \{0, 1\}$ a binary labor force participation decision and ϕ an additively separable utility cost of work that individuals incur when participating to the labor market. Assume that ϕ is distributed according to a type III extreme value distribution with cumulative distribution function $F(\cdot)$ and probability density function $f(\cdot)$, with $f' < 0$.[47] Utility is maximized subject to a budget constraint $c = \{l = 1\} \cdot z + B$, where z is labor income and B the survivor benefit. Denoting the utility-maximizing labor force participation rate with Φ, the labor force participation response to a benefit change can be written as

$$\frac{d\Phi}{dB} = -\gamma \cdot \frac{d\Phi}{dz} \cdot \frac{z}{B} \tag{6}$$

where γ is a coefficient of relative risk aversion and $d\Phi/dz$ is the labor force participation response to a wage-rate change. The latter is a negative function of the utility cost of work (ϕ). The formula shows that labor supply responses to unearned income losses are larger whenever: *(i)* utility over consumption is highly curved and the marginal utility of consumption rises sharply as consumption falls (as captured by higher values of γ); or *(ii)* the responsiveness of labor force participation to the wage rate is high, or equivalently the utility cost of adjusting labor supply is low.[48] In the following

[46] The model also assumes a constant wage rate and no credit-market constraints.

[47] A distribution with these characteristics is a Weibull distribution with shape parameter $\sigma < 1$.

[48] This result is based on Chetty [2004, 2006*b*, 2008].

sections I first provide evidence of there being adjustment costs associated to the observed labor supply response. I then develop a revealed-preference method to infer the value of the benefit from observed participation responses to benefit losses.

6.1 Evidence on Adjustment Costs

I investigate how the participation response to a given benefit drop correlates with a measure of the cost of adjusting labor supply. Evidence of a negative correlation between the labor supply response and such measure is consistent with the notion that adjustment costs are non-zero in the context analyzed. Figure 14 shows heterogeneity in the semi-elasticity of labor force participation to the benefit by different levels of the regional unemployment rate in the region where the individual resides.[49] Individual observations are binned into the quartiles of the distribution of the regional unemployment rate in each calendar year.[50] The graph shows that the participation response is larger – in absolute value – for lower levels of the unemployment rate.[51] The cost of increasing labor supply is likely to be larger when the unemployment rate is higher, either because finding a job requires more search effort or because keeping the current job requires more on-the-job effort. All in all, these results are suggestive of there being important labor supply adjustment costs, which may be especially pronounced for some groups of individuals.

6.2 A Revealed-Preference Approach for Estimating the Value of Transfers

The extent to which individuals increase work effort in response to a drop in unearned income reveals, *ceteris paribus*, the consumption value that such lost income would have provided. Larger responses must mean that the lost income is highly valued and that there are large welfare gains from recouping it. In this section, I demonstrate that the extent to which surviving spouses increase their labor supply in response to a realized drop in the survivor benefit reveals their implicit valuation of the benefit itself and can therefore be used to measure the value of transfers within the widowhood state. The value of the marginal unit of transfers (denoted by MB) is captured by the percent change in the marginal utility of consumption between the low-benefit and the high-benefit states

$$MB = \frac{u'(c(0)) - u'(c(B))}{u'(c(B))} \tag{7}$$

[49] Data on the regional unemployment rate at annual frequency are taken from ISTAT. I match each individual-year observation with the regional unemployment rate in that same year in his/her region of residence.

[50] I combine the second and third quartiles to improve the precision of the estimates.

[51] In the Italian economy, higher rates of unemployment are typically correlated with higher rates of undeclared work, which may also explain the pattern obtained in Figure 14. In order to control for the potential confounding role of the level of the black economy, I include the regional rate of undeclared work at the annual level among the regression covariates. The rate of undeclared work is measured as the ratio of estimated irregular full-time-equivalent employment over estimated total full-time-equivalent employment. Data on the rate of undeclared work are taken from ISTAT.

This ratio provides a measure of the welfare gain from transferring a unit of benefit from the high- to low-benefit state. The higher the marginal utility of consumption in the low- relative to the high-benefit state, the larger the gains from such transfer and, more generally, from increasing the generosity of survivor insurance benefits.

Proposition 1 *Let individual utility be given by $u(c) - \mathbf{I}\{l = 1\} \cdot \phi$, where c is consumption, $l \in \{0, 1\}$ a binary labor force participation decision and ϕ an additively separable utility cost of work, distributed with cumulative distribution function $F(\phi)$. Consumption c cannot exceed the sum of labor income z and the survivor benefit B. Denote the optimal level of labor force participation with Φ. Then,*

$$MB = \frac{u'(c(0)) - u'(c(B))}{u'(c(B))} \approx -\frac{\left[\frac{d\Phi}{d\log B}\right]}{\varepsilon} \tag{8}$$

where ε is the semi-elasticity of labor supply to labor earnings.

Proof. See Appendix B. ■

Proposition 1 shows that the value of the benefit can be identified by scaling the semi-elasticity of labor force participation to the benefit by the semi-elasticity of labor supply to labor earnings. If labor supply is relatively inelastic to changes in the wage rate, then larger participation responses indicate that surviving spouses have a high valuation of extra resources in the widowhood state.[52] The intuition behind this result is that the extent to which individuals undertake costly actions to increase their consumption in the low-benefit state provides a measure of the utility gain that they would get from more generous transfers. From a theoretical standpoint, by exploiting labor supply responses *within* the widowhood state, Proposition 1 allows for both state-dependent preferences and anticipation responses. By relying on optimizing behavior, revealed-preference methods work under the assumption that individuals are not subject to optimization frictions that prevent them from optimally responding along the relevant adjustment margin. They also assume the absence or separability of other margins of adjustment.

Empirical implementation. For the empirical implementation of the result in Proposition 1, I use the IV-RD estimate of the semi-elasticity of labor force participation to the benefit reported in column (2) of Table 6 and the simulated value of $\varepsilon = 0.6$ as in Blundell et al. [2016].[53] The value of $-d\Phi/d\log B$ that I estimate in the data is approximately 0.3. Rescaling it by 0.6, I obtain a measure of the value of the marginal unit of transfer equivalent to 0.5.[54] This suggests that the

[52] The model builds on previous work by Chetty [2008] and Landais [2015].

[53] Based on simulated data, Blundell et al. [2016] calculate a semi-elasticity of participation of 0.38 for single women with no children and 0.78 for lone mothers. Weighting these two elasticities by the share of survivors with and without dependent children in my sample, I obtain a weighted average of approximately 0.6.

[54] The 95 percent confidence interval of this effect ranges from 0.33 to 0.62.

marginal value of consumption is 50 percent higher among widow(er)s in the low-benefit regime as compared to widow(er)s in the high-benefit regime.

It is useful to consider how this result compares to other existing estimates in the literature. Several papers have both developed estimation methods and provided actual estimates of the welfare gain from increased welfare generosity. This has been more marked in the context of unemployment insurance (Gruber [1997], Chetty [2008], Landais [2015]), Hendren [2017] and Landais and Spinnewijn [2018]), but recent work has also focused on survivor insurance (Fadlon and Nielsen [2015, 2018]). In the context of unemployment insurance, consumption-implementation approaches that exploit the causal effect of job loss on consumption provide estimates in the ballpark of 0.2 (Gruber [1997]). More recent evidence using *ex-ante* consumption and spousal labor supply responses finds a value of unemployment insurance of approximately 0.5-0.6 (Hendren [2017]). To the best of my knowledge, the only existing estimate of the value of survivor insurance is provided by Fadlon and Nielsen [2015]. Rescaling changes in survivors' labor supply around spousal death by a measure of the utility cost of work, they estimate a value of survivor insurance that ranges from 0.03 for surviving spouses aged less than 60, to 0.94 for spouses of older ages. In comparison with existing estimates, the value of $MB = 0.5$ that I estimate appears therefore relatively high, suggesting that widowhood is a state with a high marginal utility of consumption and in which increased survivor benefit generosity would deliver substantial welfare gains.

7 Conclusion

In this paper, I provide novel estimates of the income effect of welfare transfers on individual labor supply, earnings and total income, in the context of the Italian survivor benefit program. I find that surviving spouses respond to benefit losses with one-for-one increases in earned income, implying a marginal propensity to earn out of unearned income of -1.0. This large earnings response stems from increased labor force participation, in the form of increased entry into the labor market by younger survivors and delayed retirement by older survivors. No intensive-margin nor wage rate response to the benefit drop is detected. Individuals are found to significantly increase the take-up of paid family leave and unemployment insurance benefits in response to the benefit drop. Because the presence of dependent children grants a more generous allowance, households that will experience the largest benefit drops upon loss of dependency, extend tertiary education enrolment by almost 20 percent in order to delay the benefit drop. Thus, labor force participation and program substitution both emerge as margins through which individuals increase their disposable income in response to a realized drop in unearned income.

I develop a simple model of extensive labor supply choices and demonstrate that participation responses to realized benefit losses are revealing of the implicit valuations of welfare transfers in the widowhood state. The intuition behind this result is that the extent to which individuals undertake costly actions to increase disposable income in response to a benefit drop are informative of their valuation of the extra consumption that would be provided by the benefit. According to the model's predictions, the large observed participation responses imply that widowhood is a state with high marginal utility of consumption and that substantial welfare gains could be obtained from increased survivor insurance generosity. Because it requires labor supply rather than consumption data, the revealed-preference approach that I propose is potentially widely applicable given the increasingly large availability of detailed data on individual labor supply from administrative and other sources. Moreover, being based on estimates of participation responses to unearned income, the approach can be applied to a broad class of public policies involving income transfers.

Whilst there is a selection issue of studying survivor benefit recipients, nonetheless I believe there is scope for generalizing the findings in this paper to other contexts. To the extent that widow(er)s in my sample can be representative of single parents, and single mothers in particular, my findings can be relevant to a larger set of public policies in the US and in Europe. On the other hand, the likely low probability and predictability of spousal death at younger ages implies that households are probably limitedly insured against the associated income shock. In this respect, my estimates of the income effect and of the implicit valuation of the benefit are likely to provide an upper bound of what would be estimated for "shocks" that can be anticipated and that are easier to insure *ex ante*.

The normative assessment that I draw in this paper is based on a partial equilibrium framework. A comprehensive evaluation of the welfare implications of reduced survivor insurance generosity would require an appraisal of the long-term consequences on intergenerational educational and labor market outcomes, and on individual well-being. From a general-equilibrium standpoint, it would also be important to understand how the provision of survivor insurance benefits affects decisions regarding human capital accumulation, marriage and fertility (Borella, De Nardi and Yang [2017]; Low et al. [2018]; Persson [2018]). I see these as interesting avenues for future research.

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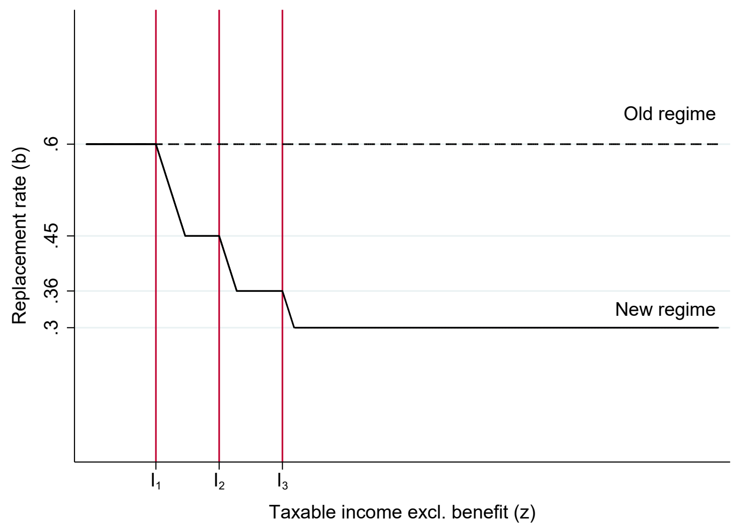
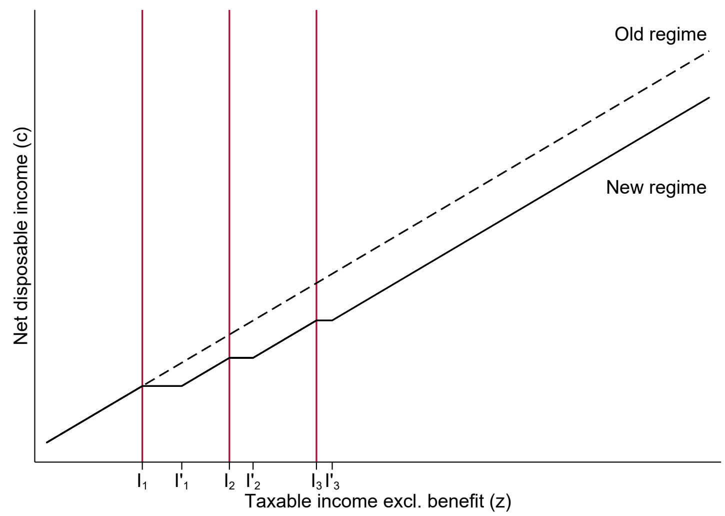
Figures

Figure 1. Benefit Replacement Rate Schedule



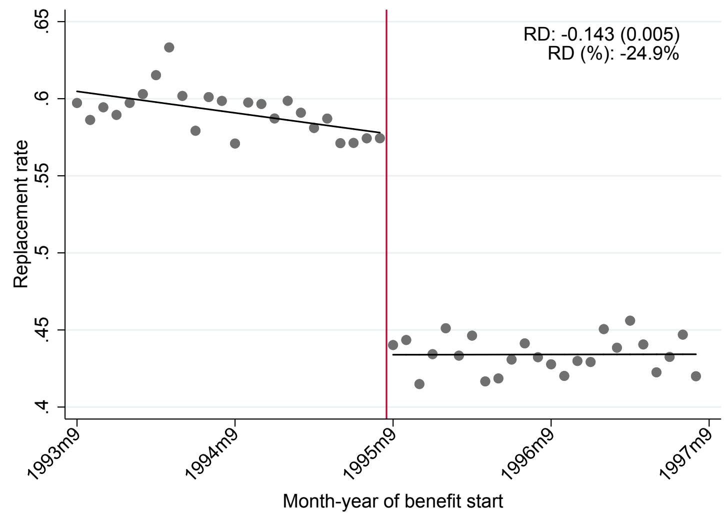
Notes: The graph reports the benefit replacement rate schedule for surviving spouses without dependent children or grandchildren in the old and new regime. The x-axis represents the surviving spouse's taxable income net of the survivor benefit (z) and the y-axis represents the survivor replacement rate (b). The dashed line refers to the old regime, while the solid line refers to the new regime. I_j for $j = 1, 2, 3$ indicates the income thresholds at which the replacement rate changes under new-regime rules: I_1 is equivalent to three times the annual minimum pension, I_2 to four times the annual minimum pension and I_3 to five times the annual minimum pension. The nominal values of the minimum pension and of its multiples for the years from 1990 to 2017 are reported in Table A1.

Figure 2. Budget Constraint



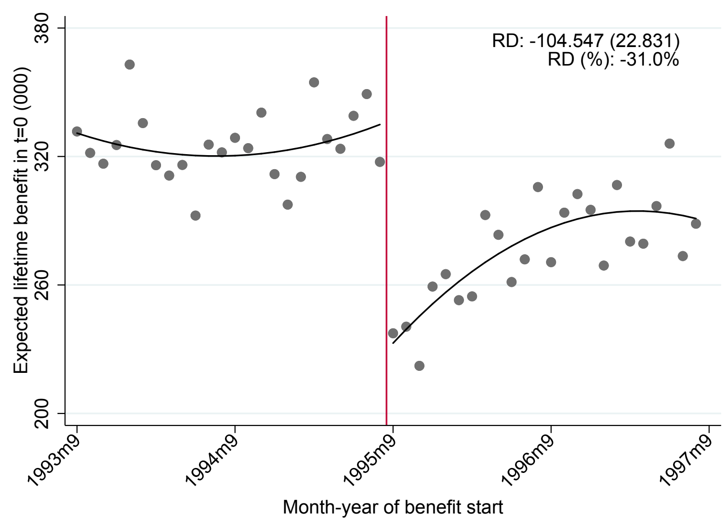
Notes: The graph illustrates the effect of the 1995 reform on the budget set of a surviving spouse without dependent children nor grandchildren in the (z, c) plane, where z indicates taxable income net of the survivor benefit and c denotes disposable income. Specifically, $c = z + B(P, b(z)) - T(z + B(P, b(z)))$, where $B(\cdot)$ is the survivor benefit, which is a function of the pension of the deceased P and of the replacement rate $b(z)$; $T(\cdot)$ is a tax function representing personal income taxes payable on taxable income including the survivor benefit $(z+B(\cdot))$. The dashed line represents the individual budget constraint under the old regime, while the solid line the individual budget constraint under the new regime. The vertical bars indicate the income brackets relevant to the determination of the benefit replacement rate in the new regime: I_1 is equivalent to three times the annual minimum pension, I_2 to four times the annual minimum pension and I_3 to five times the annual minimum pension. The thresholds $I' = (b^N_{j-1} - b^N_j)P + I_{j-1}$ $\forall j = 1, 2, 3$ indicate the convex kinks in the budget constraint. The budget constraint is constructed using the mean value of P, the income thresholds and the personal income tax parameters in effect at the time of the 1995 reform.

Figure 3. Effect of the Reform on the Benefit Replacement Rate



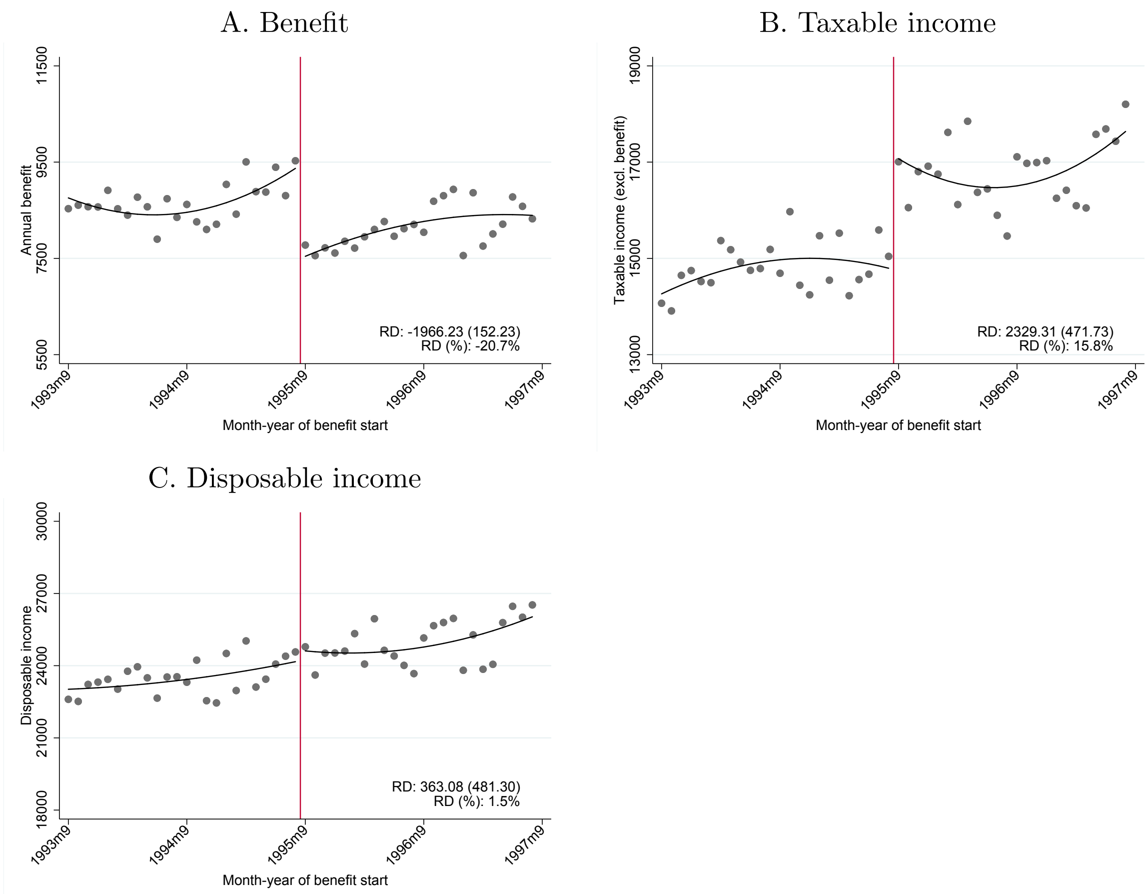
Notes: The graph shows the average replacement rate by month-of-benefit-start bin for surviving spouses with taxable income in the second or higher income brackets in the first year of benefit receipt. It also reports the coefficient η_0 and associated robust standard error from estimating equation 3, using the benefit replacement rate b in $t = 0$ as outcome variable. The estimated η_0 is also reported as a percent of the mean outcome in the control group.

Figure 4. Effect of the Reform on the Expected Lifetime Benefit



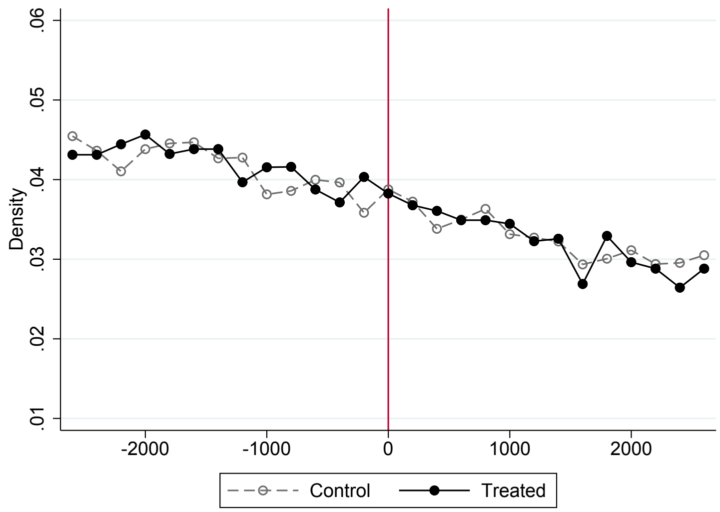
Notes: The graph shows the expected lifetime benefit by month-of-benefit-start bin for surviving spouses with taxable income in the second or higher income brackets in the first year of benefit receipt. The lifetime benefit is computed by multiplying the annual benefit in $t = 0$ by life expectancy at time $t = 0$. Life expectancy tables are obtained from the Italian Statistical Institute (ISTAT) and are split by gender, age, calendar year and region of residence. The graph also reports the coefficient β_0 and associated robust standard error from estimating equation 2, using the expected lifetime benefit as outcome variable. The estimated β_0 is also reported as a percent of the mean outcome in the control group.

Figure 5. Effect of the Reform on Annual Benefit, Taxable Income and Disposable Income



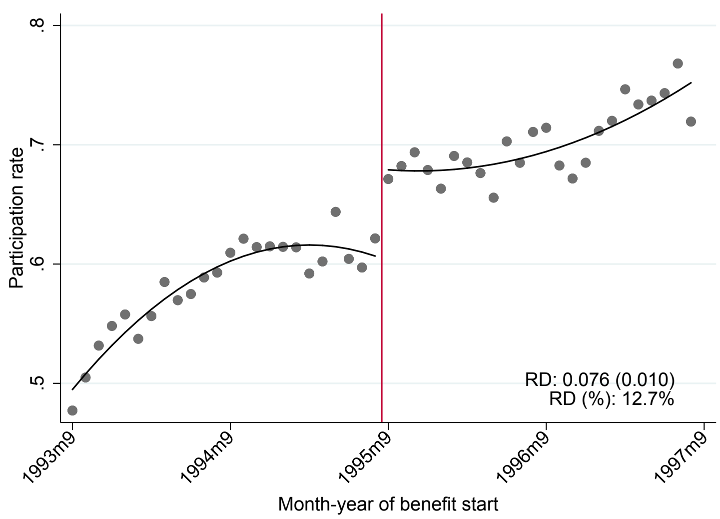
Notes: The graphs show the mean value of different outcome variables by month-of-benefit-start bin, pooling event-time years from $t = 0$ to $t = 15$. The solid dark lines display predicted values from the quadratic parametric regression in equation 3. Each graph also reports the coefficient η_0 and associated robust standard error from estimating equation 3, and the estimated η_0 as a percent of the mean outcome in the control group. Panel A refers to the annual survivor benefit B, Panel B to taxable income z and Panel C to disposable income $z + B$.

Figure 6. Empirical Density of Taxable Income around Convex Kinks by Treatment Status



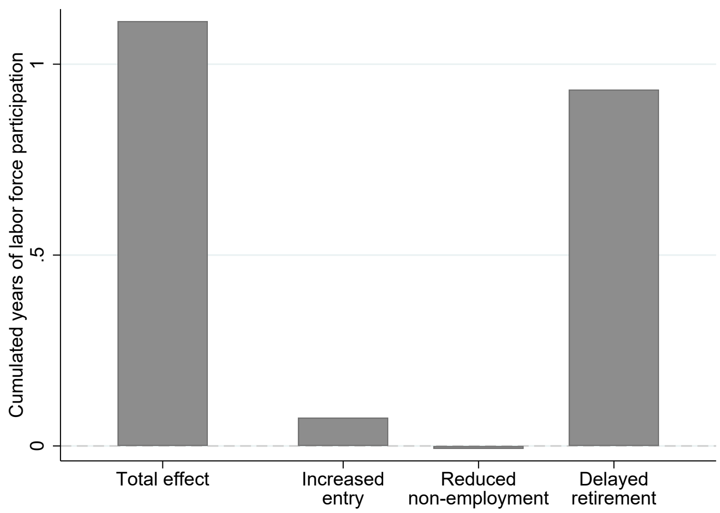
Notes: The graph plots the empirical distribution of taxable income pooling observations around the three convex kinks created by the reform and pooling all years from $t = 0$ to $t = 15$. The vertical bar represents the location of the convex kinks. Each dot refers to a €200 bin in the range $[-2,700; 2,700]$ centered around the kink. Black circles represent observations in the treatment group and hollow circles to observations in the control group.

Figure 7. Participation Response



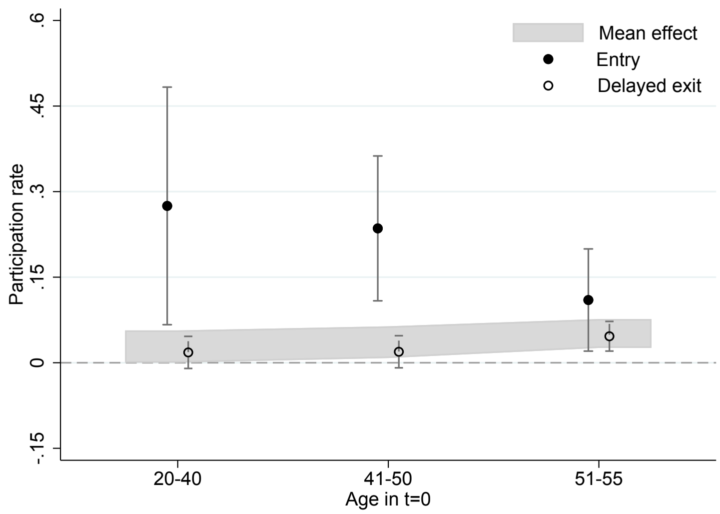
Notes: The graph shows the mean values of the participation rate in each month-of-benefit-start bin, pooling event-time years from $t = 0$ to $t = 15$. The solid dark lines display predicted values from the quadratic parametric regression in equation 3. The graph also reports the coefficient η_0 and associated robust standard error from estimating equation 3, and the estimated η_0 as a percent of the mean outcome in the control group.

Figure 8. Decomposition of Participation Response along Entry and Exit Margins



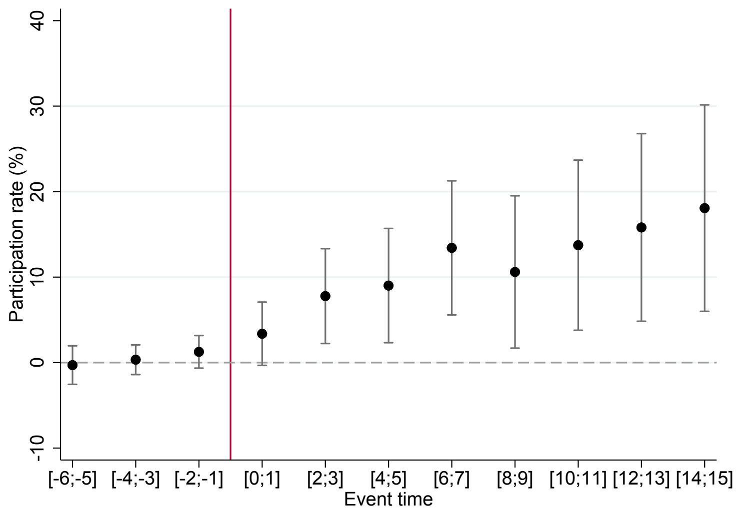
Notes: The graph shows a decomposition of the reduced-form effect on cumulated years experience between $t = 0$ and $t = 15$ along the entry and exit margin. Specifically, the first bar to the left reports reports the coefficient η_0 from estimating equation 3 using cumulated work experience in $t = 15$ as outcome variable. The remaining three bars decompose such effect into increased entry (second bar from the left) and delayed exit, distinguishing between delayed exit in the form of delayed non-employment (third bar from the left) and delayed retirement (fourth bar from the left). The second bar from the left reports the coefficient η_0 from estimating equation 3 using cumulated work experience in $t = 15$ as outcome variable for individuals who were not working in $t = -1$ and weighting the estimate by the share of such individuals in the sample. The third bar from the left reports the coefficient η_0 from estimating equation 3 using the (negative of the) number of non-employment years (excluding retirement) between $t = 0$ and $t = 15$ for individuals who were working in $t = -1$ and weighting the estimate by the share of such individuals in the sample. The fourth bar from the left is analogous to the third, but uses the number of years of retirement between $t = 0$ and $t = 15$ as outcome variable.

Figure 9. Profile of the Participation Response by Age in $t = 0$ and Employment Status in $t = -1$



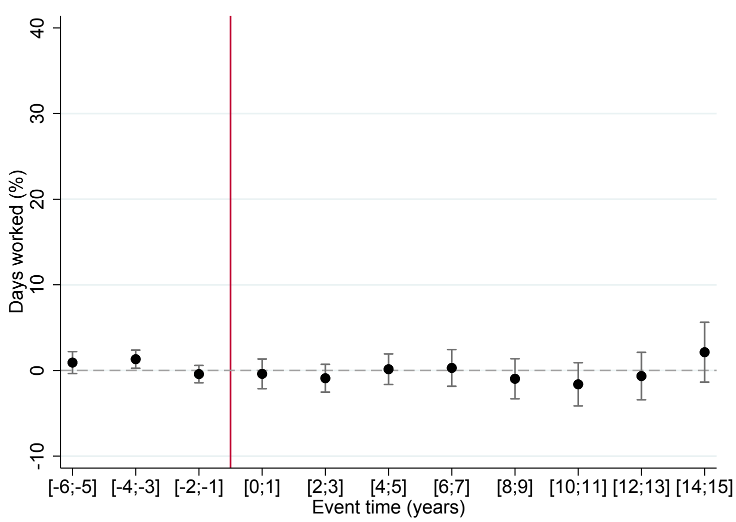
Notes: The graph outlines the profile of the labor force participation response by age in $t = 0$ and employment status in $t = -1$. The graph reports the estimated coefficient η_0 and associated 95 percent confidence interval from equation 3, using the participation rate as outcome variable and pooling event-time years from $t = 0$ to $t = 15$. The shaded area shows the 95 percent confidence interval of the coefficient η_0 for individuals in different age groups, irrespective of their employment status in $t = -1$. Black circles report the same coefficient for individuals who were not working in $t = -1$, while hollow circles for individuals who were working in $t = -1$. The capped vertical bars report 95 percent confidence intervals based on robust standard errors. The mean effect on participation (represented by the shaded area) is therefore a weighted average of an entry effect (represented by the black circles) and a delayed exit effect (represented by the hollow circles).

Figure 10. Dynamic of the Participation Response



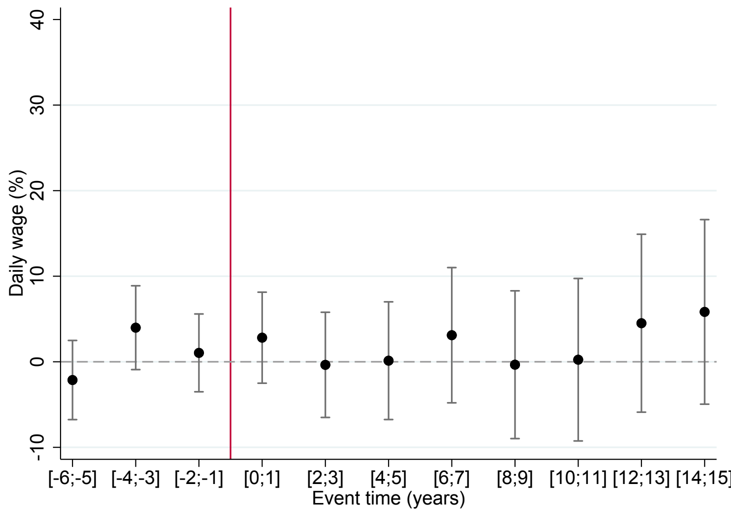
Notes: The graph reports the coefficient η_0 from estimating equation 3 using the participation rate as outcome variable and pooling event-time years from $t = -6$ to $t = 15$ into biennia. Black circles indicate the estimated η_0 in percent of the mean in the control group for different event-time years. The capped vertical bars report 95 percent confidence intervals based on robust standard errors.

Figure 11. Dynamic of the Intensive Margin Response



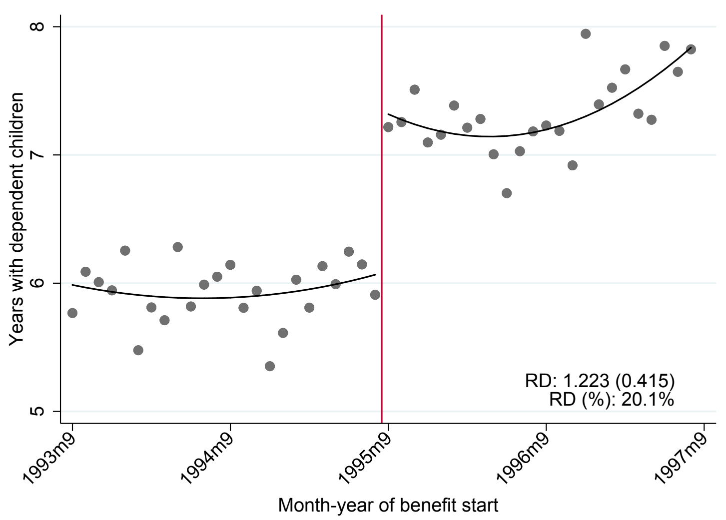
Notes: The graph reports the coefficient η_0 from estimating equation 3 using the number of days worked as outcome variable and pooling event-time years from $t = -6$ to $t = 15$ into biennia. Black circles indicate the estimated η_0 in percent of the mean in the control group for different event-time years. The capped vertical bars report 95 percent confidence intervals based on robust standard errors. The estimates are conditional on employment and on work experience in $t = 0$.

Figure 12. Dynamic of the Wage Rate Response



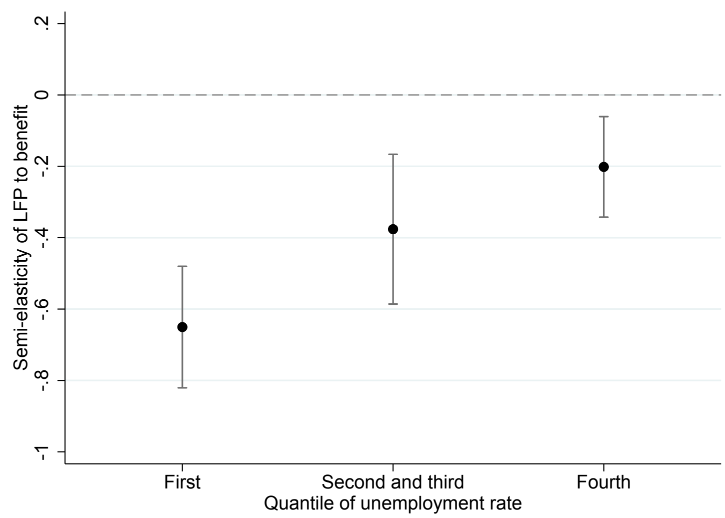
Notes: The graph reports the coefficient η_0 from estimating equation 3 using the daily wage rate as outcome variable and pooling event-time years from $t = -6$ to $t = 15$ into biennia. Black circles indicate the estimated η_0 in percent of the mean in the control group for different event-time years. The capped vertical bars report 95 percent confidence intervals based on robust standard errors. The estimates are conditional on employment and on work experience in $t = 0$. The wage rate is computed as annual earnings divided by the number of days worked.

Figure 13. Effect of the Reform on the Number of Years with Dependent Children



Notes: The graph shows the mean values of the number of years with dependent children in each month-of-benefit-start bin. The solid dark lines display predicted values from the quadratic parametric regression in equation 3. The graph also reports the coefficient η_0 and associated robust standard error from estimating equation 3, and the estimated η_0 as a percent of the mean outcome in the control group.

Figure 14. Heterogeneous Treatment Effects by Regional Unemployment Rate in t



Notes: The graph reports the coefficient β from estimating equation 1 using an indicator for labor force participation as outcome and $logB_{it}$ as main regressor. Black circles indicate the estimated β and the capped vertical bars report 95 percent confidence intervals based on robust standard errors. The graph shows heterogeneity in the semi-elasticity of labor force participation to the benefit by different quartiles of the distribution of the regional unemployment rate, in the region in which the surviving spouse resides. Data on the regional unemployment rate are at annual frequency and are taken from ISTAT. Individual-year observations are matched with the regional unemployment rate in the same year in the region where the individual resides. Individual observations are then binned into the quartiles of the distribution of the regional unemployment rate in each year. To improve the precision of the estimates, the second and third quartiles are combined. In order to control for the potential confounding role of the level of the black economy, I include the regional rate of undeclared work – as measured by estimated irregular full-time-equivalent employment over estimated total full-time-equivalent employment – at the annual level among the regression covariates. Data on the rate of undeclared work are taken from ISTAT.

Tables

Table 1. Benefit Replacement Rates

	Benefit start date	
	Before Sept 1, 1995	After Sept 1, 1995
	(1)	(2)
Spouse (with and without dependent children)		
Spouse only		
Survivor's taxable income $\leq 3 \times$ minimum pension	60%	60%
Survivor's taxable income $\leq 4 \times$ minimum pension	60%	45%
Survivor's taxable income $\leq 5 \times$ minimum pension	60%	36%
Survivor's taxable income $> 5 \times$ minimum pension	60%	30%
Spouse with one dependent child or grandchild	80%	80%
Spouse with two or more dependent children or grandchildren	100%	100%
Dependent children (absent the spouse)		
One dependent child or grandchild	60%	70%
Two dependent children or grandchildren	80%	80%
Three or more dependent children or grandchildren	100%	100%
Dependent parents or siblings (absent the spouse, children or grandchildren)		
Each dependent relative	15%	15%

Notes: The table reports the benefit replacement rates for different types of survivors and separately for benefits with start date before or after September 1995. Dependent children and grandchildren aged 18-21 who are high-school students and not working are entitled to the benefit up to age 21. University students up to age 26 are also entitled to the benefit, provided that they are not working. Children, grandchildren, parents or siblings that are disabled or incapacitated are also considered dependent. Each parent or sibling receives 15% of the pension of the deceased, up to 100%.

Table 2. Effect of the Reform on the Benefit Amount in $t = 0$

	Regression discontinuity				Control mean
	(1)	(2)	(3)	(4)	(5)
Predicted second or higher income bracket					
Benefit in $t = 0$	-1510.21***	-1684.83***	-2137.66***	-1963.66***	8494.83
	(260.413)	(296.800)	(376.689)	(407.618)	
Lifetime benefit (000)	-67.032***	-85.273***	-99.641***	-104.547***	337.387
	(13.811)	(16.691)	(20.155)	(22.831)	
Obs.	13556	13556	13556	13556	-
Full sample					
Benefit in $t = 0$	-465.171***	-558.993***	-593.922***	-602.776***	8371.92
	(73.548)	(85.830)	(109.989)	(120.938)	
Lifetime benefit (000)	-18.917***	-24.567***	-23.623***	-25.120***	298.57
	(3.243)	(3.916)	(4.879)	(5.511)	
Observations	94578	94578	94578	94578	-
Benefit-start-month FE		x		x	-
Calendar year FE		x		x	-
Linear trend	x	x	x	x	-
Quadratic trend			x	x	-

Notes: The table reports the coefficient β_0 from estimating equation 2 using the benefit amount in $t = 0$ as outcome variable. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. Columns (1) and (2) are based on a linear parametric specification, without and with controls respectively. Columns (3) and (4) are based on a quadratic parametric specification, without and with controls respectively. Column (5) reports the mean of the outcome variable in the control group. All estimates are based on a 24-month symmetric bandwidth. The top panel reports estimates for the sample with predicted second or higher income bracket. The bottom panel reports estimates for the full sample. The lifetime benefit is computed by multiplying the annual benefit in $t = 0$ by life expectancy at time $t = 0$. Life expectancy tables are obtained from the Italian Statistical Institute (ISTAT) and are split by gender, age, calendar year and region of residence. The lifetime benefit is in thousands of euros.

Table 3. Effect of the Reform on Benefit, Taxable Income and Disposable Income

	Regression discontinuity				Control mean
	(1)	(2)	(3)	(4)	(5)
Benefit	-1155.25***	-1306.96***	-1771.21***	-1966.23***	9462.31
	(103.033)	(110.320)	(145.140)	(152.225)	
Taxable income	1674.92***	1473.23***	2508.59***	2329.31***	14470.64
	(380.664)	(407.731)	(455.254)	(471.733)	
Disposable income	519.674	166.277	737.385	363.081	23932.95
	(386.337)	(414.151)	(464.363)	(481.298)	
Observations	216896	216896	216896	216896	-
Benefit-start-month FE		x		x	-
Calendar year FE		x		x	-
Linear trend	x	x	x	x	-
Quadratic trend			x	x	-

Notes: The table reports the coefficient η_0 from estimating equation 3 using different outcome variables and pooling event-time years from $t = 0$ to $t = 15$. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. Columns (1) and (2) are based on a linear parametric specification, without and with controls respectively. Columns (3) and (4) are based on a quadratic parametric specification, without and with controls respectively. Column (5) reports the mean of the outcome variable in the control group. All estimates are based on a 24-month symmetric bandwidth.

Table 4. IV Estimate of the Effect of the Benefit on Taxable Income and Disposable Income

	Taxable income	Disposable income	Taxable income	Disposable income
	(1)	(2)	(3)	(4)
Benefit	-1.205***	-0.205	-1.008***	-0.008
	(0.337)	(0.337)	(0.303)	(0.303)
Observations	216896	216896	216896	216896
Benefit-start-month FE	x	x	x	x
Calendar year FE	x	x	x	x
Linear trend	x	x	x	x
Quadratic trend			x	x

Notes: The table reports the IV-RD coefficient β from estimating equation 1 using different outcome variables and pooling event-time years from $t = 0$ to $t = 15$. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. The IV estimates in columns (1) and (2) are based on a first stage with linear parametric specification, while those in columns (3) and (4) on a first stage with quadratic parametric specification with individual controls. All estimates are based on a 24-month symmetric bandwidth.

Table 5. Effect of the Reform on Labor Supply, Retirement and Other Work-Related Outcomes

	Regression discontinuity				Control mean	Observations
	(1)	(2)	(3)	(4)	(5)	(6)
Participation rate	0.019***	0.024***	0.071***	0.076***	0.603	216896
	(0.007)	(0.007)	(0.010)	(0.010)		
Cumulated experience in $t = 15$	0.307	0.570*	1.061**	1.113**	10.256	13556
	(0.285)	(0.325)	(0.421)	(0.466)		
Retirement rate in $t = 15$	-0.012	-0.041	-0.072*	-0.079*	0.516	13556
	(0.025)	(0.029)	(0.037)	(0.041)		
Days worked	0.147	-0.248	-1.169	-1.561	351.482	123829
	(0.995)	(1.033)	(1.491)	(1.521)		
Daily wage	0.382	-0.427	0.998	-0.047	76.755	123829
	(0.818)	(0.860)	(1.217)	(1.251)		
Full-time job	-0.020**	-0.027***	-0.004	-0.013	0.891	68253
	(0.005)	(0.005)	(0.007)	(0.008)		
Change firm	-0.008	-0.012	0.005	0.003	0.082	68253
	(0.007)	(0.008)	(0.010)	(0.011)		
Change industry	0.001	0.001	0.011*	0.012*	0.029	68253
	(0.004)	(0.005)	(0.007)	(0.007)		
Change province	-0.008*	-0.011***	-0.005	-0.008	0.025	68253
	(0.004)	(0.004)	(0.006)	(0.006)		
Benefit-start-month FE		x		x	-	-
Calendar year FE		x		x	-	-
Linear trend	x	x	x	x	-	-
Quadratic trend			x	x	-	-

Notes: The table reports the coefficient η_0 from estimating equation 3 using different outcome variables and pooling event-time years from $t = 0$ to $t = 15$. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. Columns (1) and (2) are based on a linear parametric specification, without and with controls respectively. Columns (3) and (4) are based on a quadratic parametric specification, without and with controls respectively. Column (5) reports the mean of the outcome variable in the control group and column (6) the number of observations. All estimates are based on a 24-month symmetric bandwidth. Cumulated experience and the retirement rate are measured at event-time $t = 15$. The wage rate is computed as annual earnings divided by the number of days worked. The probability of holding a full-time job, changing firm, changing industry (at three-digit level) and changing province are estimated on the sample of individuals employed in the private sector. The estimates for days worked, the wage rate, the probability of holding a full-time job, changing firm, changing industry and changing province are all conditional on employment and include the number of years of work experience in $t = 0$ among the individual controls.

Table 6. IV Estimates of the Effect of the Benefit on Labor Supply, Program Substitution and Dependency Period

	Benefit (000) (1)	ln Benefit (2)	Control mean	Observations
Participation rate	-0.040***	-0.286***	0.603	216896
	(0.006)	(0.043)		
Cumulated experience in $t = 15$	-0.842**		10.256	13556
	(0.414)			
Retirement rate in $t = 15$	0.100**		0.516	13556
	(0.047)			
Days worked	2.563***		351.482	123829
	(0.731)			
Daily wage	2.210***		76.755	123829
	(0.618)			
Full-time job	0.010*		0.891	68253
	(0.005)			
Change firm	-0.004		0.082	68253
	(0.005)			
Change industry	-0.002		0.029	68253
	(0.003)			
Change province	-0.000		0.025	68253
	(0.003)			
Paid family leave	-0.003**		0.008	117264
	(0.002)			
Paid sick leave	-0.003		0.043	117264
	(0.003)			
Unemployment benefits	-0.017***		0.017	117264
	(0.003)			
Dependency period	-0.653**		6.095	5595
	(0.272)			
Benefit-start-month FE	x	x	-	-
Calendar year FE	x	x	-	-
Linear trend	x	x	-	-
Quadratic trend	x	x	-	-

Notes: The table reports the IV-RD coefficient β from estimating equation 1 using different outcome variables and pooling event-time years from $t = 0$ to $t = 15$. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. Estimates of the first stage are based on a quadratic parametric specification with individual controls and a 24-month symmetric bandwidth. Cumulated experience and the retirement rate are measured at event-time $t = 15$. The wage rate is computed as annual earnings divided by the number of days worked. The probability of holding a full-time job, changing firm, changing industry (at three-digit level) and changing province are estimated on the sample of individuals employed in the private sector. The estimates for days worked, the wage rate, the probability of holding a full-time job, changing firm, changing industry and changing province are all conditional on employment and include the number of years of work experience in $t = 0$ among the individual controls. Estimates for paid family leave, paid sick leave and unemployment benefits are conditional on employment in t or $t - 1$, and include the number of years of work experience in $t = 0$ among the individual controls. The benefit amount is in thousands of euros.

Table 7. Effect of the Reform on Social Insurance Take-up

	Regression discontinuity				Control mean
	(1)	(2)	(3)	(4)	(5)
Paid family leave	0.004*	0.005*	0.011***	0.012***	0.008
	(0.002)	(0.002)	(0.003)	(0.004)	
Paid sick leave	0.019***	0.016***	0.020***	0.016**	0.043
	(0.005)	(0.005)	(0.007)	(0.007)	
Unemployment benefits	-0.004	0.002	0.007	0.013***	0.017
	(0.003)	(0.003)	(0.004)	(0.005)	
Observations	117264	117264	117264	117264	-
Benefit-start-month FE		x		x	-
Calendar year FE		x		x	-
Linear trend	x	x	x	x	-
Quadratic trend			x	x	-

Notes: The table reports the coefficient η_0 from estimating equation 3 using different outcome variables and pooling event-time years from $t = 0$ to $t = 15$. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. Columns (1) and (2) are based on a linear parametric specification, without and with controls respectively. Columns (3) and (4) are based on a quadratic parametric specification, without and with controls respectively. Column (5) reports the mean of the outcome variable in the control group. All estimates are based on a 24-month symmetric bandwidth. All estimates are conditional on employment in t or $t-1$, and include the number of years of work experience in $t = 0$ among the individual controls.

Table 8. Effect of the Reform on the Benefit upon Loss of Dependency and on the Dependency Period

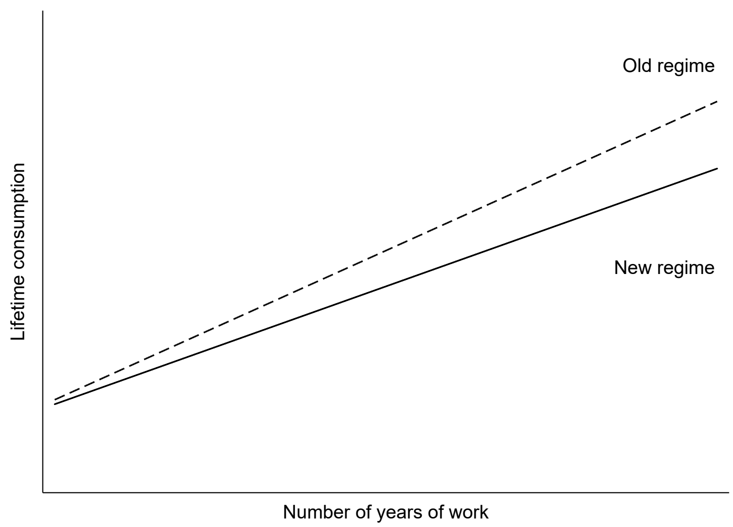
	Regression discontinuity				Control mean
	(1)	(2)	(3)	(4)	(5)
Benefit upon dep. loss	-1242.33***	-1186.79***	-1508.89***	-1305.21***	7875.19
	(261.391)	(287.243)	(388.340)	(408.394)	
Dependency period	0.737***	1.255***	1.220***	1.223***	6.095
	(0.222)	(0.290)	(0.335)	(0.415)	
Observations	5595	5595	5595	5595	-
Benefit-start-month FE		x		x	-
Calendar year FE		x		x	-
Linear trend	x	x	x	x	-
Quadratic trend			x	x	-

Notes: The table reports the coefficient η_0 from estimating equation 3 using different outcome variables. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. Columns (1) and (2) are based on a linear parametric specification, without and with controls respectively. Columns (3) and (4) are based on a quadratic parametric specification, without and with controls respectively. Column (5) reports the mean of the outcome variable in the control group. All estimates are based on a 24-month symmetric bandwidth. The benefit is measured in the year after all children have lost their dependency status. The dependency period is measured as the number of years with dependent children within the household.

Appendices

A Additional Figures and Tables

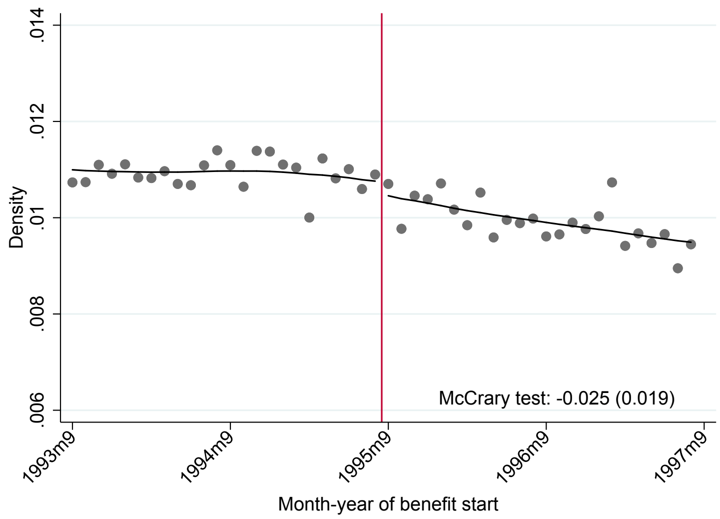
Figure A1. Dynamic Framework



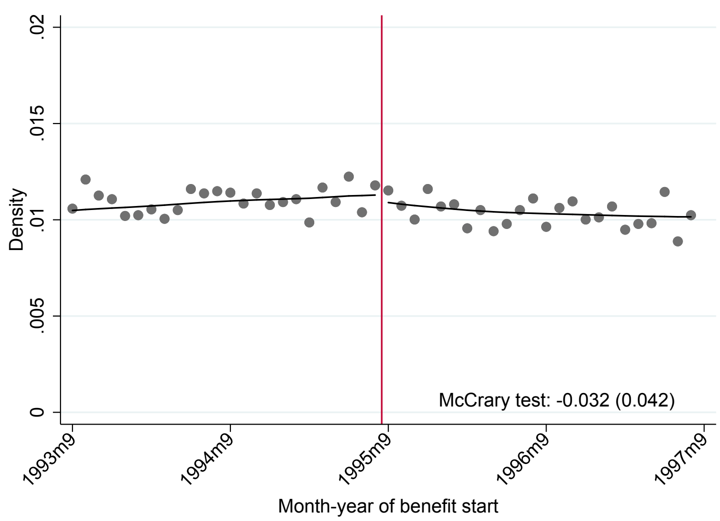
Notes: The graph illustrates the effect of the 1995 reform on the dynamic budget set of a surviving spouse without dependent children nor grandchildren. The x-axis reports the number of years of work out of the total number of available years. The y-axis reports the level of lifetime consumption associated to each number of years of work. Without loss of generality, the graph is constructed under the assumption that, when working, individuals earn a fixed annual wage and receive 30 percent of their spouses's pension as survivor benefit; when not working, individuals do not earn any wage and receive 60 percent of their spouse's pension as survivor benefit. The dashed line represents the individual lifetime budget constraint under the old regime, while the solid line the individual lifetime budget constraint under the new regime.

Figure A2. Distribution of Benefits by Start Date and McCrary Tests

A. Full sample

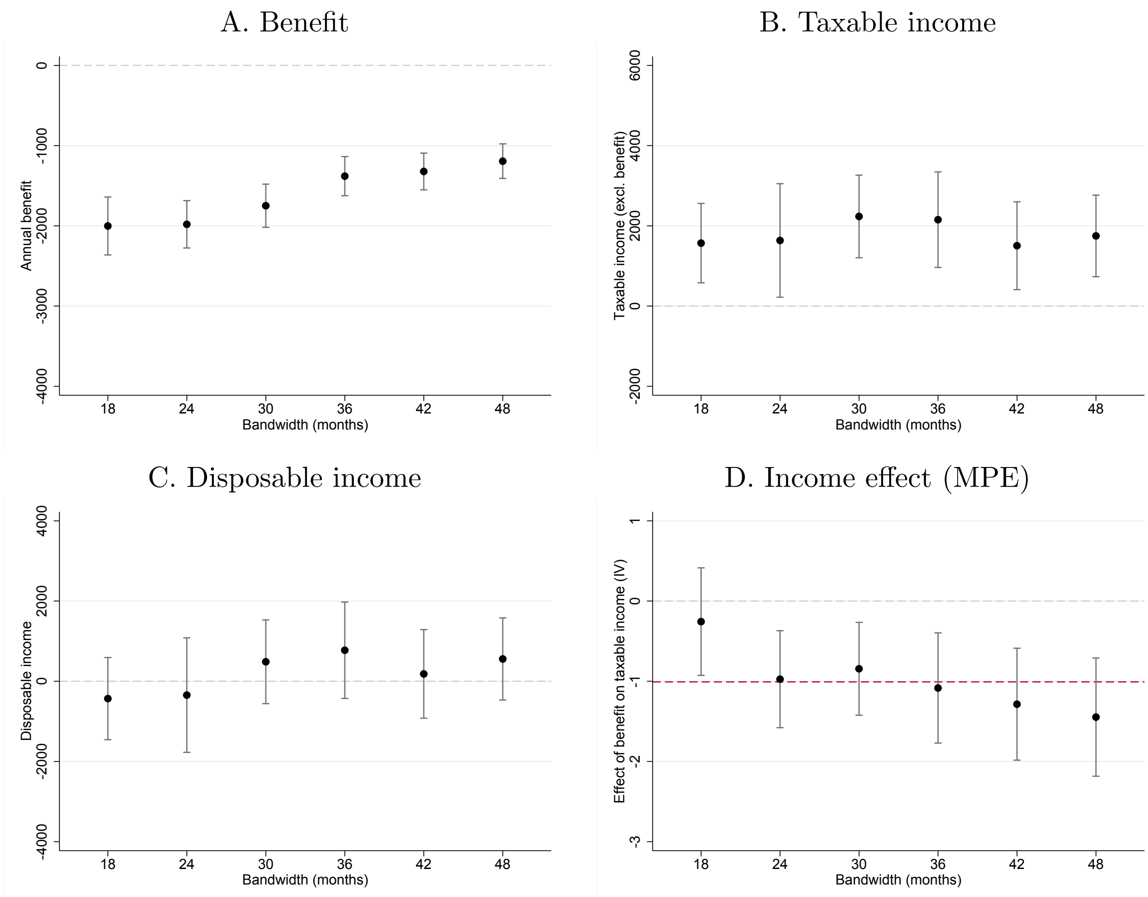


B. Individuals with taxable income in second or higher income bracket



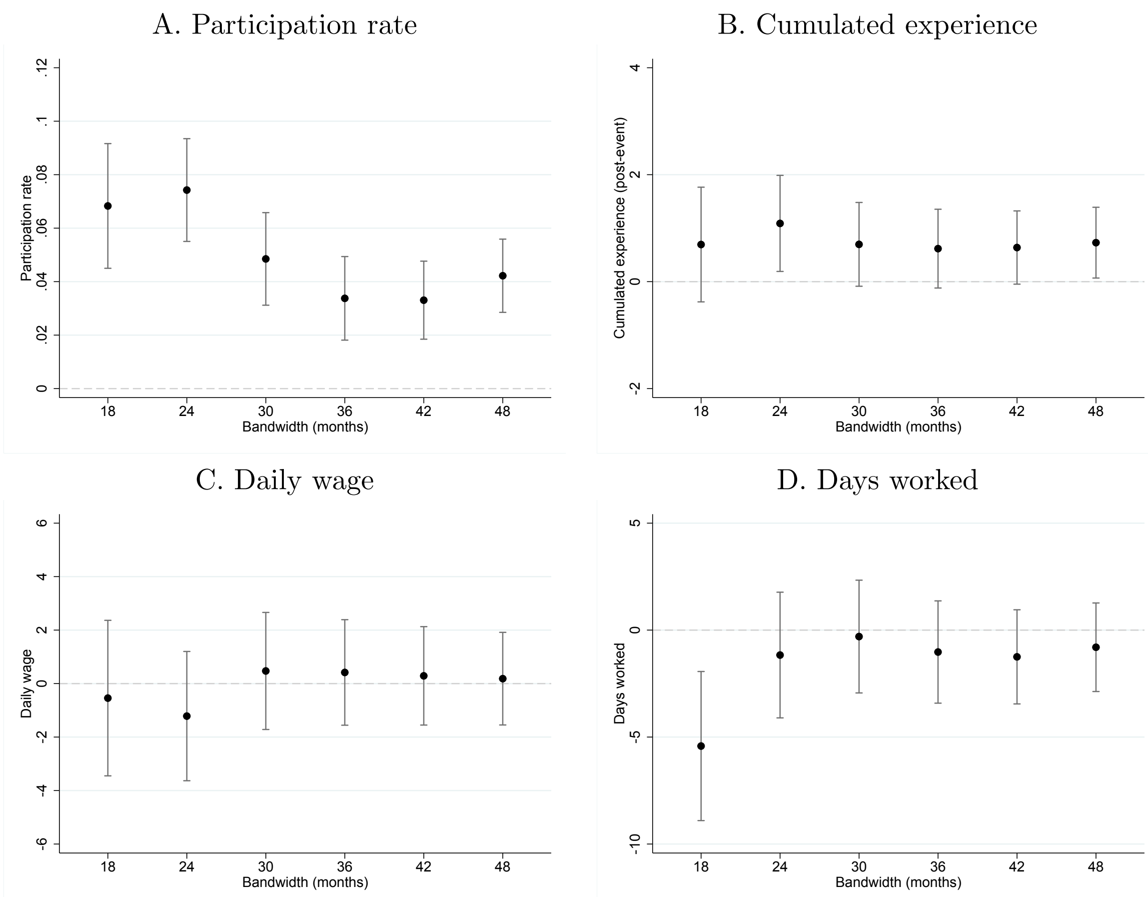
Notes: The graphs plot the empirical probability density function of benefit recipients by month-year of benefit start for the entire sample (Panel A) and for the subgroup of individuals with taxable income in the second or higher income bracket in $t = 0$ (Panel B). Each graph reports the test statistics and associated standard error in parenthesis of a McCrary test of the discontinuity in the probability density function of the running variable at the September 1995 threshold.

Figure A3. RD Coefficients and Confidence Intervals by Bandwidth



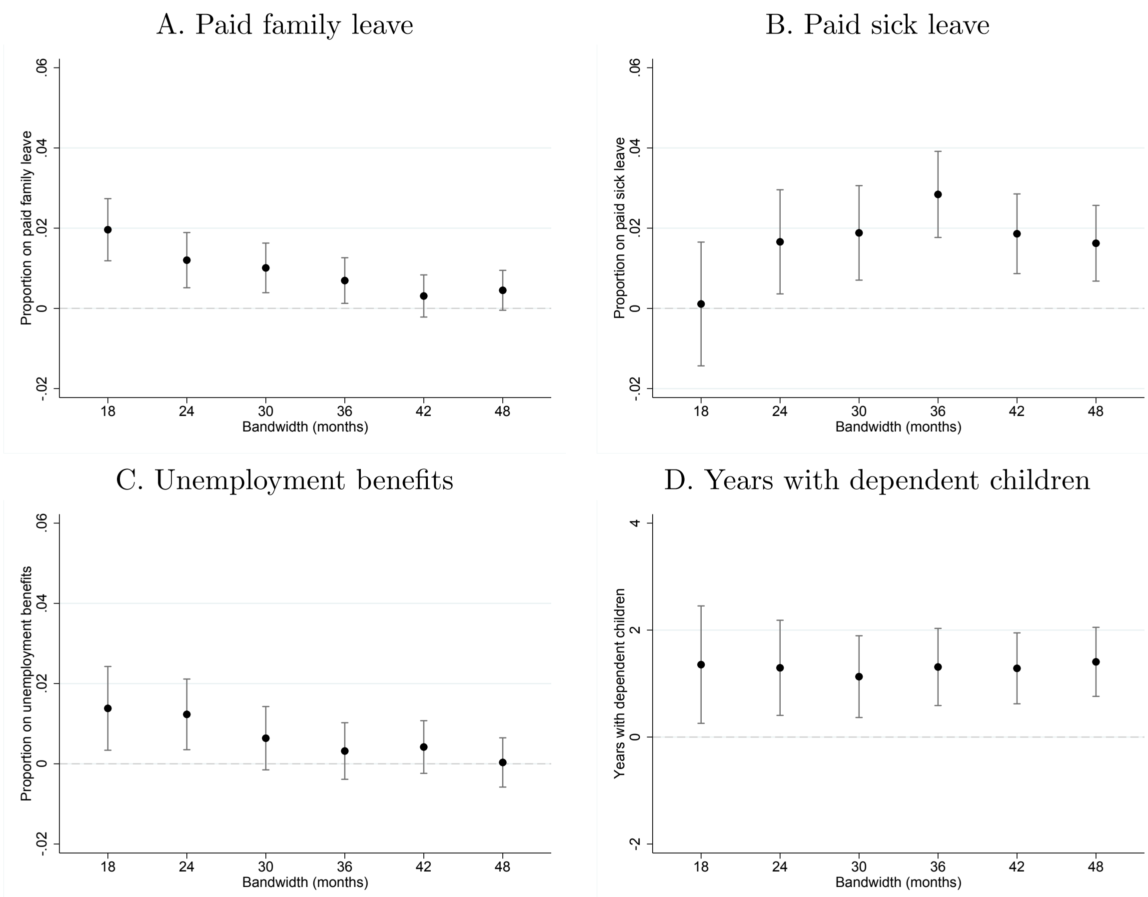
Notes: Panels A, B and C report the coefficient η_0 from estimating equation 3 using a quadratic parametric specification and different bandwidths. Panel D reports the coefficient β from an IV estimation of equation 1 using a quadratic parametric specification for the first stage and different bandwidths. Solid circles indicate the estimated coefficients. The capped vertical bars report 95 percent confidence intervals based on robust standard errors.

Figure A4. RD Coefficients and Confidence Intervals by Bandwidth



Notes: The graphs report the coefficient η_0 from estimating equation 3 using a quadratic parametric specification and different non-parametric local linear regression. Solid circles indicate the estimated η_0 for specifications with different symmetric bandwidths. The capped vertical bars report 95 percent confidence intervals based on robust standard errors.

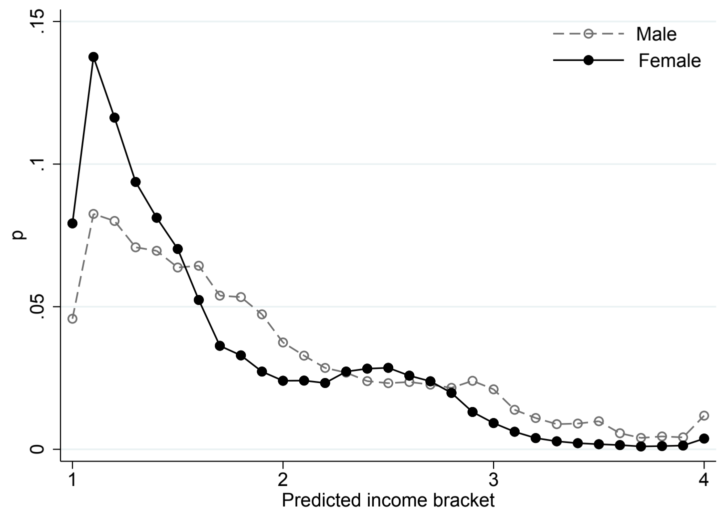
Figure A5. RD Coefficients and Confidence Intervals by Bandwidth



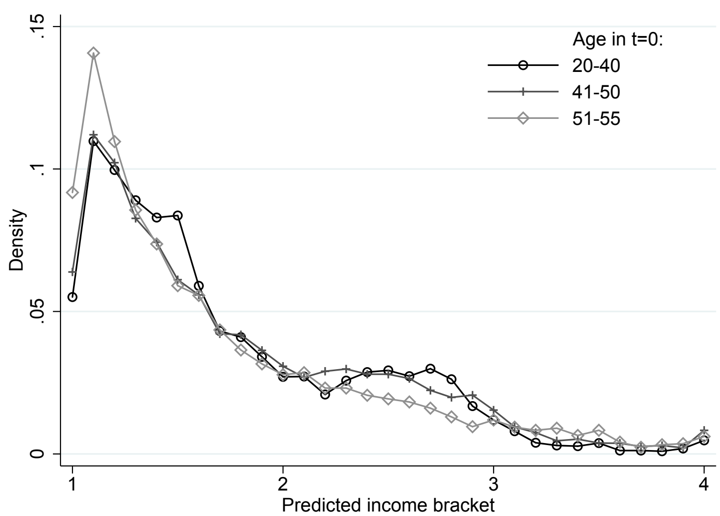
Notes: The graphs report the coefficient η_0 from estimating equation 3 using a quadratic parametric specification and different non-parametric local linear regression. Solid circles indicate the estimated η_0 for specifications with different symmetric bandwidths. The capped vertical bars report 95 percent confidence intervals based on robust standard errors.

Figure A6. Empirical Density of Predicted Taxable Income Bracket by Gender and Age in $t = 0$

A. Gender

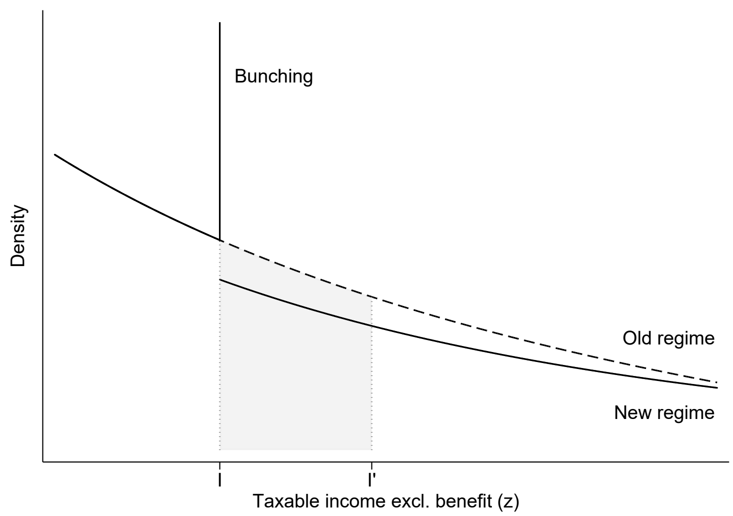


B. Age in $t = 0$



Notes: The graphs report the empirical distribution of predicted taxable income bracket by gender (Panel A) and age at event time $t = 0$ (Panel B). The graph reports the distribution for individuals with predicted taxable income in the second or higher income bracket. Each dot refers to a 0.1 bin.

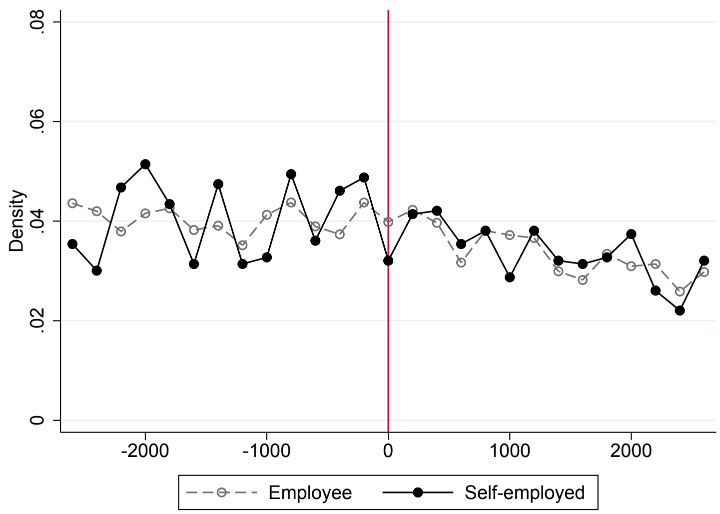
Figure A7. Density of Taxable Income



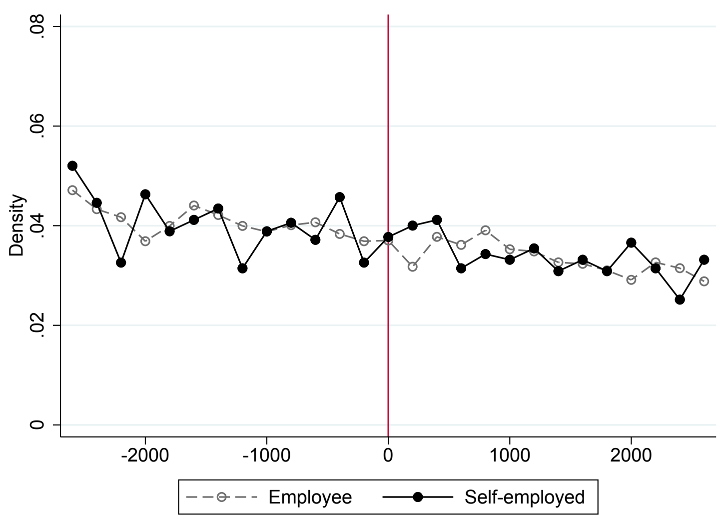
Notes: The graph plots a theoretical density function of taxable income z. The dashed line illustrates the case of a smooth density function. By introducing a discrete change in the marginal tax rate, the reform creates a convex kink in the budget constraint of treated individuals at $z = I$. Absent the kink, individuals would locate smoothly along the old-regime budget set generating a smooth taxable income density. Once introduced, the convex kink creates a disincentive for individuals to locate in the range $[I, I']$ and induces individuals who would counterfactually locate in that range to bunch at I. This behavior will give rise to excess bunching in the taxable income density function at the kink point and a left-shift in the density above the kink, as illustrated by the solid line and the shadowed region in the graph.

Figure A8. Empirical Density of Taxable Income around Convex Kinks by Treatment Status and Employment Status

A. Treatment group

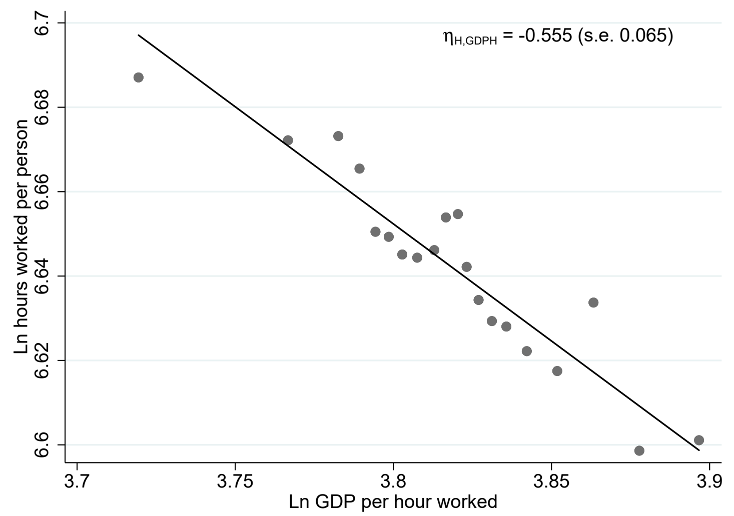


B. Control group



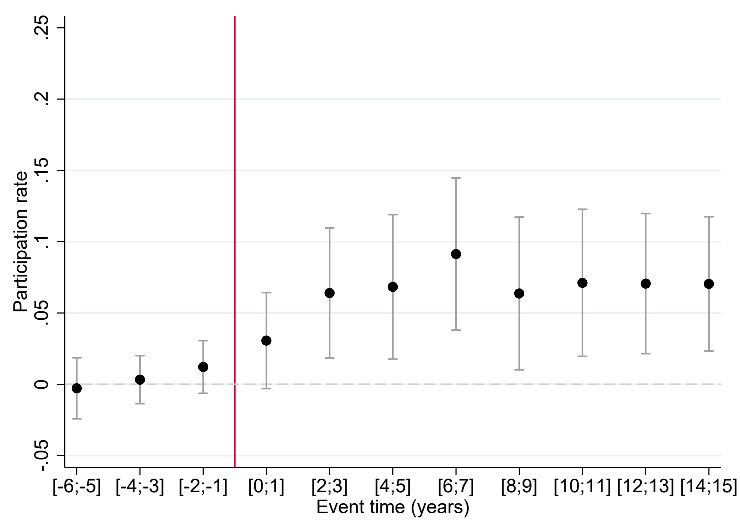
Notes: The graphs plot the empirical distribution of taxable income pooling observations around the three convex kinks created by the reform and pooling all years from $t = 0$ to $t = 15$. The vertical bar represents the location of the convex kinks. Each dot refers to a €200 bin in the range $[-2,700; 2,700]$ centered around the kink. Black circles represent self-employed individuals, while hollow circles represent wage earners. Panel A is based on observations in the treatment group and Panel B to observations in the control group.

Figure A9. Macro-Elasticity of Hours Worked per Person to GDP per Hour



Notes: The graph reports a binned scatter plot of the logarithm of hours of work per person on the logarithm of GDP per hour, controlling for country and calendar year fixed effects. The plot is based OECD data at the country-year level from 1985 to 2015 for the following countries: Australia, Belgium, Canada, Germany, Denmark, Finland, France, the United Kingdom, Italy, Japan, the Netherlands and the United States. The graph also reports the estimated elasticity of hours worked to GDP per hour and its associated robust standard error. Gray circles represent binned observations and the black line the regression fitted line.

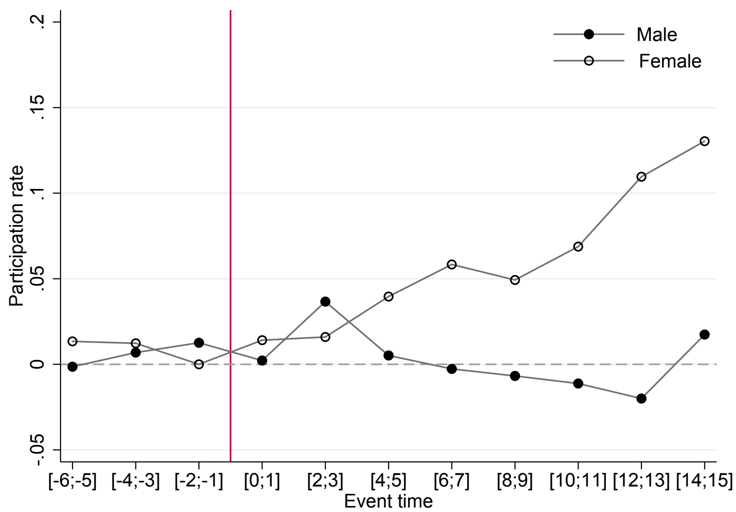
Figure A10. Dynamic of the Participation Response (Level Effect)



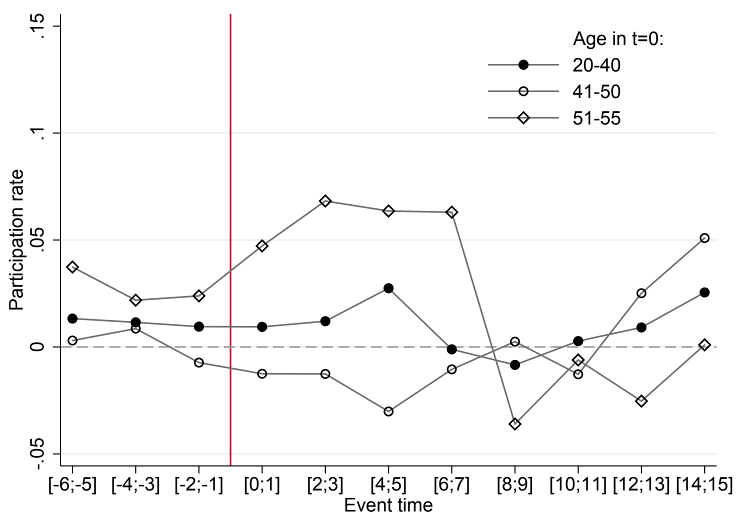
Notes: The graph reports the coefficient η_0 from estimating equation 3 using the participation rate as outcome variable and pooling event-time years from $t = -6$ to $t = 15$ into biennia. Black circles indicate the estimated η_0 for different event-time years. The capped vertical bars report 95 percent confidence intervals based on robust standard errors.

Figure A11. Heterogeneous Dynamic Effects of Labor Force Participation by Gender and Age in $t = 0$

A. Gender

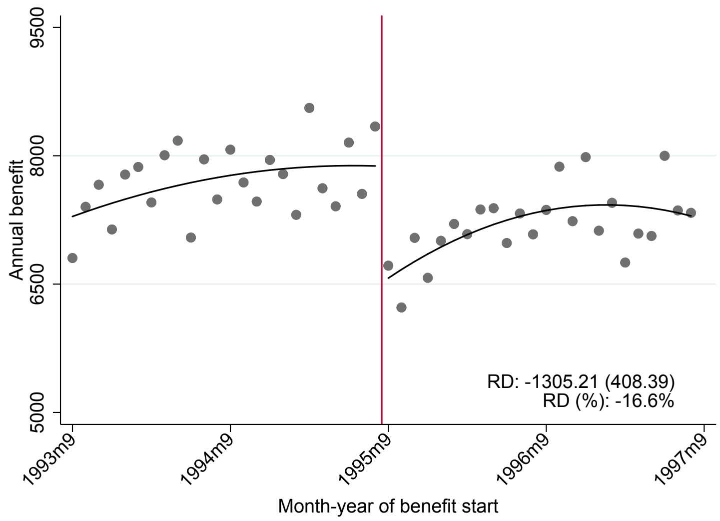


B. Age in $t = 0$



Notes: The graphs report the coefficient η_0 from estimating equation 3 using the participation rate as outcome variable and pooling event-time years from $t = -6$ to $t = 15$ into biennia. Markers indicate the estimated η_0 for each event-time year. Panel A shows heterogeneity in the dynamic of the participation response by gender. Panel B shows heterogeneity in the dynamic of the participation response by age in $t = 0$.

Figure A12. Effect of the Reform on the Benefit Upon Loss of Dependency Status



Notes: The graph shows the mean value of the annual benefit by month-of-benefit-start bin. The benefit is measured in the year after all children have lost their dependency status. The solid dark lines display predicted values from the quadratic parametric regression in equation 3. The graph also reports the coefficient η_0 and associated robust standard error from estimating equation 3, and the estimated η_0 as a percent of the mean outcome in the control group. The estimates are based on the sample of individuals with dependent children in $t = 0$ and with predicted taxable income in the second or higher income brackets.

Table A1. Annual Minimum Pension (in Euro)

Year	Amount	$\times$ 3	$\times$ 4	$\times$ 5
1990	3,142.98	9,428.93	12,571.90	15,714.88
1991	3,354.98	10,064.94	13,419.93	16,774.91
1992	3,696.41	11,089.23	14,785.64	18,482.06
1993	3,825.68	11,477.04	15,302.72	19,128.40
1994	4,006.80	12,020.41	16,027.21	20,034.01
1995	4,205.95	12,617.84	16,823.79	21,029.74
1996	4,433.21	13,299.64	17,732.86	22,166.07
1997	4,606.10	13,818.29	18,424.39	23,030.49
1998	4,684.32	14,052.95	18,737.26	23,421.58
1999	4,768.58	14,305.73	19,074.30	23,842.88
2000	4,844.78	14,534.34	19,379.12	24,223.89
2001	4,970.67	14,912.00	19,882.66	24,853.33
2002	5,104.97	15,314.91	20,419.88	25,524.85
2003	5,227.56	15,682.68	20,910.24	26,137.80
2004	5,358.34	16,075.02	21,433.36	26,791.70
2005	5,465.59	16,396.77	21,862.36	27,327.95
2006	5,558.54	16,675.62	22,234.16	27,792.70
2007	5,669.82	17,009.46	22,679.28	28,349.10
2008	5,760.56	17,281.68	23,042.24	28,802.80
2009	5,950.88	17,852.64	23,803.52	29,754.40
2010	5,992.61	17,977.83	23,970.44	29,963.05
2011	6,076.59	18,229.77	24,306.36	30,382.95
2012	6,246.89	18,740.67	24,987.56	31,234.45
2013	6,440.59	19,321.77	25,762.36	32,202.95
2014	6,517.94	19,553.82	26,071.76	32,589.70
2015	6,524.57	19,573.71	26,098.28	32,622.85
2016	6,524.57	19,573.71	26,098.28	32,622.85
2017	6,524.57	19,573.71	26,098.28	32,622.85

Notes: The table reports the nominal value of the minimum pension and of its multiples for the years from 1990 to 2017. The minimum pension is a minimum amount provided by the social security to pensioners whose pension benefit is below a subsistence income threshold. The minimum pension level is set by law each year.

Table A2. Summary Statistics for the Full Sample of Surviving Spouses

	Full sample		Treatment group		Control group	
	Mean	SD	Mean	SD	Mean	SD
Female	0.90	0.30	0.91	0.29	0.90	0.30
Age in $t = 0$	46.85	7.19	46.88	7.11	46.83	7.26
Prop. aged < 40 in $t = 0$	0.16	0.37	0.16	0.37	0.16	0.37
Prop. aged 40-50 in $t = 0$	0.45	0.50	0.46	0.50	0.44	0.50
Prop. aged 51-55 in $t = 0$	0.39	0.49	0.38	0.49	0.40	0.49
Prop. with dependent children in $t = 0$	0.45	0.50	0.44	0.50	0.46	0.50
Age of dependent children in $t = 0$	13.16	5.14	13.22	5.15	13.10	5.14
Prop. in first bracket in $t = 0$	0.86	0.34	0.85	0.36	0.88	0.33
Prop. in second bracket in $t = 0$	0.07	0.25	0.08	0.27	0.06	0.23
Prop. in third bracket in $t = 0$	0.03	0.18	0.04	0.19	0.03	0.16
Prop. in fourth bracket in $t = 0$	0.04	0.19	0.04	0.20	0.04	0.19
Prop. ever employed in $t \leq -1$	0.81	0.39	0.81	0.39	0.81	0.40
Years of experience in $t = -1$	14.25	10.12	14.31	10.18	14.20	10.06
Prop. employed in $t = -1$	0.40	0.49	0.40	0.49	0.40	0.49
Prop. employed in private sector in $t = -1$	0.60	0.49	0.60	0.49	0.60	0.49
Prop. employed in public sector in $t = -1$	0.06	0.24	0.06	0.25	0.06	0.23
Prop. employed in para-public sector in $t = -1$	0.02	0.15	0.02	0.15	0.02	0.15
Prop. self-employed in $t = -1$	0.31	0.46	0.30	0.46	0.32	0.47
Prop. in professional occupation in $t = -1$	0.00	0.06	0.00	0.07	0.00	0.06
Labor income in $t = -1$	6237.35	10761.88	6205.20	10745.04	6266.57	10777.19
Daily wage in $t = -1$	47.27	67.47	47.10	43.00	47.43	83.50
Days worked in $t = -1$	327.30	88.52	325.94	90.26	328.53	86.90
Benefit in $t = 0$	9691.92	7597.10	9712.08	7358.12	9673.44	7333.20
Income of deceased in $t = 0$	16256.70	14759.14	17322.87	14849.46	15043.89	14561.44
Pension of deceased in $t = 0$	12668.17	10407.14	13233.03	10223.97	12148.19	10546.34
Observations	94578		45022		49556	

Notes: The table reports summary statistics for the full balanced sample of surviving spouses. The statistics are computed on the sample of survivors whose benefit start date is within a 24-month symmetric bandwidth around September 1, 1995. Monetary quantities are expressed in 2010 prices. Labor income is unconditional on employment. Days worked and the wage rate are conditional on employment. The wage rate is computed as annual earnings divided by the number of days worked.

Table A3. Covariate Balancing Tests

	Regression discontinuity					Control
	(1)	(2)	(3)	(4)	(5)	mean
Female	0.003	0.005	0.006	-0.001	0.003	0.899
	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)	
Age in $t = 0$	0.070	-0.097	-0.160	-0.216	-0.075	46.860
	(0.094)	(0.124)	(0.143)	(0.179)	(0.120)	
Experience in $t = -1$	-0.001	-0.006	-0.418*	-0.188	-0.289	14.445
	(0.143)	(0.189)	(0.216)	(0.269)	(0.123)	
Earnings in $t = -1$	-269.993*	-170.699	-140.946	-105.928	-111.621	6373.42
	(139.426)	(185.312)	(211.708)	(265.219)	(224.762)	
Prop. employed in $t = -1$	0.002	-0.005	-0.002	-0.005	0.004	0.397
	(0.006)	(0.008)	(0.010)	(0.012)	(0.011)	
Days worked in $t = -1$	-0.999	-1.181	-3.594*	-0.214	0.165	341.026
	(1.358)	(1.797)	(2.007)	(2.539)	(3.165)	
Daily wage in $t = -1$	-1.282	1.675	1.189	-0.953	-3.376	47.544
	(1.020)	(1.721)	(1.353)	(2.469)	(2.204)	
Prop. on defined benefit	-0.005	-0.013	-0.006	-0.011	-0.003	0.312
	(0.007)	(0.009)	(0.010)	(0.012)	(0.009)	
Observations	94578	94578	94578	94578	94578	-
Month-of-benefit-start FE		x		x		-
Calendar year FE		x		x		-
Linear trend	x	x	x	x		-
Quadratic trend			x	x		-
LLR					x	-

Notes: The table reports the coefficient η_0 from estimating equation 3 for different outcome variables. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. Columns (1) and (2) are based on a linear parametric specification, without and with controls respectively. Columns (3) and (4) are based on a quadratic parametric specification, without and with controls respectively. Column (5) is based on non-parametric local linear regression. Column (6) reports the mean of the outcome variable in the control group. All estimates are based on a 24-month symmetric bandwidth. Earnings are measured unconditional on employment. The number of days worked and the wage rate are conditional on employment. The wage rate is computed as annual earnings divided by the number of days worked.

Table A4. Summary Statistics for the Sample of Surviving Spouses with Predicted Taxable Income in the Second or Higher Income Bracket

	Full sample		Treatment group		Control group	
	Mean	SD	Mean	SD	Mean	SD
Female	0.64	0.48	0.66	0.48	0.62	0.48
Age in $t = 0$	43.50	7.49	43.56	7.31	43.45	7.65
Prop. aged < 40 in $t = 0$	0.29	0.45	0.28	0.45	0.30	0.46
Prop. aged 40-50 in $t = 0$	0.51	0.50	0.53	0.50	0.49	0.50
Prop. aged 51-59 in $t = 0$	0.20	0.40	0.19	0.39	0.21	0.41
Prop. with dependent children in $t = 0$	0.58	0.49	0.58	0.49	0.59	0.49
Age of dependent children in $t = 0$	12.23	5.61	12.29	5.61	12.18	5.62
Prop. ever employed in $t \leq -1$	1.00	0.05	1.00	0.04	1.00	0.06
Years of experience in $t = -1$	20.81	8.85	20.83	8.75	20.78	8.94
Prop. employed in $t = -1$	0.96	0.19	0.96	0.18	0.96	0.19
Prop. employed in private sector in $t = -1$	0.61	0.49	0.60	0.49	0.62	0.48
Prop. employed in public sector in $t = -1$	0.14	0.35	0.15	0.36	0.14	0.34
Prop. employed in para-public sector in $t = -1$	0.06	0.24	0.06	0.24	0.06	0.23
Prop. self-employed in $t = -1$	0.17	0.38	0.17	0.38	0.17	0.38
Prop. in professional occupation in $t = -1$	0.01	0.09	0.01	0.10	0.01	0.09
Labor income in $t = -1$	24216.42	12681.93	24096.99	12625.93	24328.48	12734.13
Daily wage in $t = -1$	72.36	40.08	71.86	38.14	72.82	41.82
Days worked in $t = -1$	347.55	53.53	346.83	55.00	348.22	52.10
Benefit in $t = 0$	10670.52	9974.44	10437.28	9605.68	10892.00	10308.06
Income of deceased in $t = 0$	21361.10	21933.74	21886.54	20968.13	20589.71	23261.99
Pension of deceased in $t = 0$	14104.45	13660.71	14528.82	12980.38	13701.51	14265.97
Observations	13556		6562		6994	

Notes: The table reports summary statistics for the balanced sample of surviving spouses with predicted taxable income in the second or higher income bracket. The statistics are computed on the sample of survivors whose benefit start date is within a 24-month symmetric bandwidth around September 1, 1995. Monetary quantities are expressed in 2010 prices. Labor income is unconditional on employment. Days worked and the wage rate are conditional on employment. The wage rate is computed as annual earnings divided by the number of days worked.

Table A5. Heterogeneous Effects by Gender and Age in $t = 0$

	Gender		Age in $t = 0$		
	Female	Male	20-40	41-50	51-55
	(1)	(2)	(3)	(4)	(5)
Benefit	-1984.11***	-734.445***	-2840.77***	-1194.09***	-2099.00***
	(208.525)	(89.437)	(174.841)	(245.582)	(294.558)
	[11318.84]	[7129.74]	[8842.95]	[9612.85]	[8944.35]
MPE	-1.325***	-0.106	-1.097***	-0.999	-0.451
	(0.376)	(0.772)	(0.459)	(0.644)	(0.299)
Participation rate	0.101***	0.045***	0.028**	0.036***	0.051***
	(0.012)	(0.017)	(0.014)	(0.014)	(0.013)
	[0.639]	[0.553]	[0.883]	[0.585]	[0.212]
Days worked	1.045	-0.092	6.307***	-3.999	3.418
	(2.668)	(3.855)	(2.820)	(3.534)	(5.871)
	[341.04]	[338.62]	[347.63]	[336.31]	[326.39]
Daily wage	1.673	-5.854**	1.359	0.526	-1.944
	(1.412)	(2.487)	(1.848)	(1.871)	(3.396)
	[74.238]	[83.820]	[73.630]	[80.669]	[80.453]
Benefit-start-month FE	x	x	x	x	x
Calendar year FE	x	x	x	x	x
Linear trend	x	x	x	x	x
Quadratic trend	x	x	x	x	x

Notes: The table reports the estimated coefficient η_0 from equation 3, for various outcome variables and groups of survivors, pooling event-time years from $t = 0$ to $t = 15$. The second row reports instead the estimated coefficient β from equation 1 using taxable income as outcome variable. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. The mean value of the outcome variable in the control group is reported in square brackets. The wage rate is computed as annual earnings divided by the number of days worked. The estimates for days worked and the wage rate are all conditional on employment and include the number of years of work experience in $t = 0$ among the individual controls.

Table A6. IV Estimate of the Effect of the Benefit on Taxable Income and Disposable Income

	Taxable income (1)	Disposable income (2)	Taxable income (3)	Disposable income (4)
Benefit	-0.943**	0.057	-0.847**	0.153
	(0.450)	(0.450)	(0.419)	(0.419)
Observations	73783	73783	73783	73783
Benefit-start-month FE	x	x	x	x
Calendar year FE	x	x	x	x
Linear trend	x	x	x	x
Quadratic trend			x	x

Notes: The table reports the IV-RD coefficient β from estimating equation 1 using different outcome variables and pooling event-time years from $t = 0$ to $t = 15$. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. The IV estimates in columns (1) and (2) are based on a first stage with linear parametric specification, while those in columns (3) and (4) on a first stage with quadratic parametric specification with individual controls. All estimates are based on a 24-month symmetric bandwidth. Individuals with observed taxable income in the second and third income bracket are excluded from the estimation sample.

Table A7. Placebo Test for the Effect of the Reform on the Dependency Period

	Number of years with dependent children
Placebo thresholds	
September 1992	-0.404
	(0.568)
September 1993	0.757
	(0.423)
September 1994	-1.317***
	(0.413)
September 1995	1.223***
	(0.415)
September 1996	-0.345
	(0.421)
September 1997	0.390
	(0.416)
September 1998	-0.502
	(0.540)
Benefit-start-month FE	x
Calendar year FE	x
Linear trend	x
Quadratic trend	x

Notes: The table reports the coefficient η_0 from estimating equation 3 using different cutoff dates τ. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. Estimates are based on a quadratic parametric specification with individual controls and a 24-month symmetric bandwidth. The dependency period is measured as the number of years with dependent children within the household.

Table A8. Summary Statistics for the Sample of EITC Recipients in the March Current Population Survey (1993-1997)

	All household heads		Single household heads	
	Mean	SD	Mean	SD
Female	0.53	0.50	0.85	0.36
Age	35.47	8.56	35.18	8.44
Prop. aged < 40	0.68	0.47	0.69	0.46
Prop. aged 40-50	0.27	0.44	0.27	0.44
Prop. aged 51-59	0.05	0.22	0.04	0.20
Prop. with dependent children	0.93	0.26	0.92	0.27
Age of dependent children	7.89	4.70	8.47	4.68
Prop. single	0.51	0.50		
Prop. employed in $t = -1$	0.93	0.25	1.00	0.00
Prop. employed in private sector in $t = -1$	0.79	0.41	0.82	0.38
Prop. employed in public sector in $t = -1$	0.12	0.32	0.13	0.33
Prop. self-employed in $t = -1$	0.10	0.30	0.05	0.22
Total income (excl. unearned)	22066.68	14261.54	23397.75	13192.35
Estimated EITC amount	1557.91	1053.41	1645.41	1045.20
Observations	28036		13600	

Notes: The table reports summary statistics for the sample of EITC recipients in the March Current Population Survey (CPS) for the years 1992-1997. The sample is restricted to household heads aged 18-55 at the time of the survey. The first two columns report the mean and standard deviation of variables for all household heads, while the last two columns report the same statistics for the sample of single household heads (i.e. those for whom no cohabiting spouse is recorded in the data). Observations are weighted using the CPS individual weights. Monetary quantities have been converted in 2010 euros using the US CPI and euro/dollar purchasing-power-parity conversions.

B Proof of Proposition 1

This section shows that the semi-elasticity of participation to the benefit, rescaled by the semi-elasticity of labor supply to labor earnings, can be used to estimate the welfare gain of increasing survivor benefits in the widowhood state.

I develop a model in which widow(er)s choose labor supply at the extensive margin. Preferences are defined over consumption and labor. When participating in the labor market, individuals incur an additively separable utility cost ϕ and earn labor income z. Let utility be given by

$$u(c) - \mathbf{I}\{l = 1\} \cdot \phi \tag{B1}$$

where $u(\cdot)$ is a concave utility function, c is consumption, $l \in \{0, 1\}$ a binary labor force participation decision and ϕ labor disutility. ϕ is distributed with probability density function $f(\phi)$ and cumulative distribution function $F(\phi)$. Assuming that labor force participation generates income z, the budget constraint is

$$c = \mathbf{I}\{l = 1\} \cdot z + B \tag{B2}$$

where B is the survivor benefit.

Let $V(z, l, B)$ denote the indirect utility function. Individuals decide to work if and only if

$$V(z, 1, B) - V(0, 0, B) \geq \phi \tag{B3}$$

which is equivalent to a threshold rule whereby individuals work if and only if $\phi \leq \overline{\phi}(z, B)$. The probability of working – i.e. the labor force participation rate – is $\Phi(z, B) = F(\overline{\phi}(z, B))$.

The semi-elasticity of labor supply with respect to the benefit is

$$\frac{d\Phi}{d \log B} = f(\overline{\phi}) \cdot \frac{\partial \overline{\phi}}{\partial B} \cdot B = f(\overline{\phi}) \cdot \left[\frac{\partial V(z, 1, B)}{\partial B} - \frac{\partial V(0, 0, B)}{\partial B} \right] \cdot B \tag{B4}$$

Using a first order Taylor expansion around $z = 0$, we have

$$\frac{d\Phi}{d \log B} \approx f(\overline{\phi}) \cdot \frac{\partial^2 V}{\partial z \partial B} \cdot z \cdot B \tag{B5}$$

Since $\dfrac{\partial V}{\partial z} = u'(c(B))$ is the marginal utility of income, we have

$$\frac{d\Phi}{d\log B} \approx f(\overline{\phi}) \cdot \frac{\partial u'(c(B))}{\partial B} \cdot z \cdot B = f(\overline{\phi}) \cdot u''(c(B)) \cdot z \cdot B \tag{B6}$$

Rescaling the above expression by the semi-elasticity of labor force participation to labor earnings $\varepsilon = \dfrac{d\Phi}{d\log z} = f(\overline{\phi}) \cdot u'(c(B)) \cdot z$ and applying a first order Taylor expansion around $B = 0$, we obtain

$$\frac{\left[\dfrac{d\Phi}{d\log B}\right]}{\varepsilon} \approx \frac{u''(c(B)) \cdot B}{u'(c(B))} \approx \frac{u'(c(B)) - u'(c(0))}{u'(c(B))} \tag{B7}$$

or equivalently, the negative of the labor supply response to $\log B$ divided by ε provides a measure of the marginal benefit (MB) of survivor insurance:

$$MB = \frac{u'(c(0)) - u'(c(B))}{u'(c(B))} \approx -\frac{\left[\dfrac{d\Phi}{d\log B}\right]}{\varepsilon} \tag{B8}$$